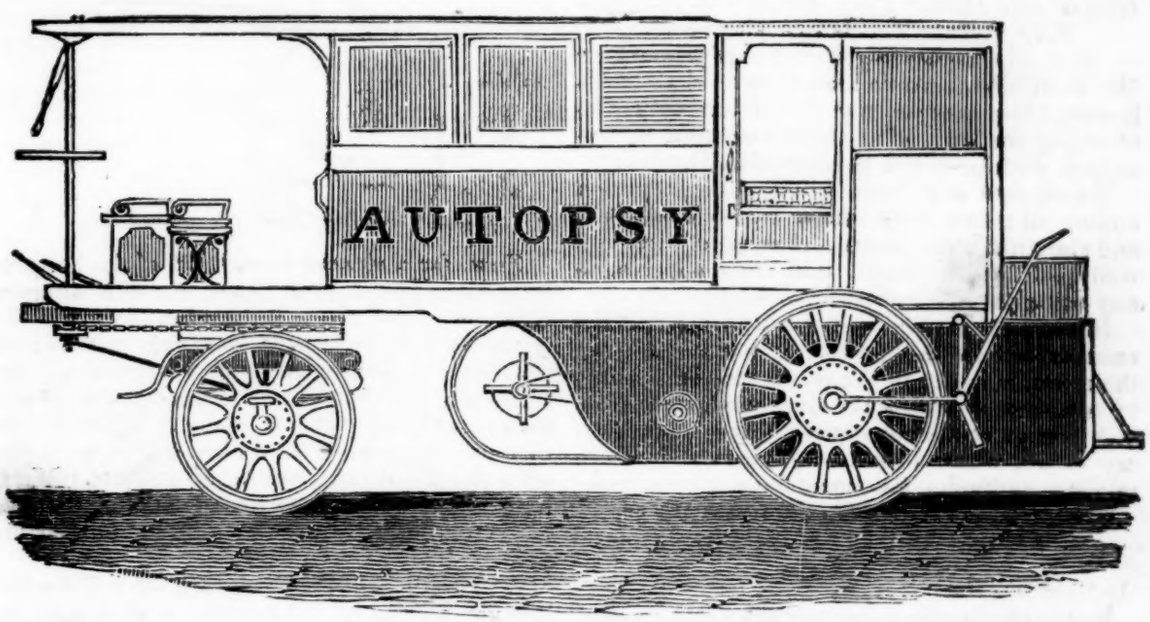
MECHANICS' MAGAZINE,

AND

REGISTER OF INVENTIONS AND IMPROVEMENTS.

VOLUME III.] MARCH, 1834. [NUMBER 3.

Books that you may carry to the fire, and hold readily in your hand, are the most useful after all. A man will often look at them, and be tempted to go on, when he would have been frightened at books of a large size, and of a more erudite appearance. —DR. JOHNSON.



HANCOCK'S STEAM-CARRIAGE "AUTOPSY."—We give an engraving of Mr. Hancock's last new steam-carriage, the "Autopsy,"—the same which performed the journey to Brighton and back, with so much success, about three weeks ago, and which has been running daily, during all the present week, between Finsbury square and Pentonville. We have witnessed its performances repeatedly, both as passengers and as lookers-on—and only re-echo the general opinion, when we say that it works admirably. The machinery, which is constructed on the improved plan, for which Mr. Hancock's last patent was taken out, will be found fully described in a subsequent communication from Mr. Hancock himself.

The quantity of coke expended in each journey of (about) two miles, scarcely exceeds a bushel; so that even supposing the tear and wear were to be as great as in the case of Mr. G. Stephenson's engines on the Liverpool and Manchester Railway, or even twice or thrice as great, the returns from such a carriage, running between the City and Pentonville or Islington, must be sufficiently ample to afford a very handsome profit. According to the following calculation, with which we were favored by a fellow-passenger, there would be a clear gain, during a period of 365 days, of nearly cent. per cent.

CAPITAL REQUIRED TO BE INVESTED.

Cost of steam-carriage - - - - - - -	£700
Spare ditto, to use when the other is under repair, - - - - - - - - - - - -	700
	£1,400

Dr.

Wages—engineer 40s. per week, steersman 30s. assistant 20s. - - - - - -	£234
Repairs - - - - - - - - - - - - - -	150
Tolls 4d. each journey × 12 × 365 - -	73
Coke 6d. per journey × 12 × 365 - -	109
Water - - - - - - - - - - - - - -	50
Rent of coach office and coach house -	100
Clerk - - - - - - - - - - - - - -	50
Premium to the patentee, at the rate (say of 1d. per passenger) 12 × 24 × 365	438
Reserve Fund to replace carriage when worn out—probably in 3 or 4 years -	175
	1,452
Dividend of £84 on £1,400 - - - - - -	1,176
	£2,628

Cr.

By twelve journeys per day, and twelve passengers each way, at 6d. each, 12 × 24 × 365, - - - - - - - - - - £2,628

We have received the following communication from the agent of Mr. Harris, respecting his invention, and on the same day the letter from our friend Archimedes. We have often stated that our columns are open to controversy on scientific subjects, if conducted without personalities, and we cheerfully insert both articles. We hope to receive other letters on the same subject.—[Ed. Mec. Mag.]

Harris' new Patent Twin Steamboat. To the Editor of the Mechanics' Magazine.

Sir,—In requesting the favor of you to give Mr. Harris' communication a place in your columns, I beg leave to say that I am only discharging the duty of an agent, without the remotest wish or desire to injure Mr. Burden.

There has not been a dissenting opinion among all those with whom I have consulted, and shown Harris' plans, and some of them are well qualified to judge, that it is superior to any thing yet discovered for velocity.

His invention admits of many advantages not enumerated in the following letter. Among them are, security of the *inner* wheels in a heavy sea, double wheels for river navigation, &c.

A model of the construction may be seen at my office, where I invite capitalists and others to come and judge for themselves.

D. Mallory, Chester's Buildings,
No. 1 Dey street.

D. Mallory, Esq.:

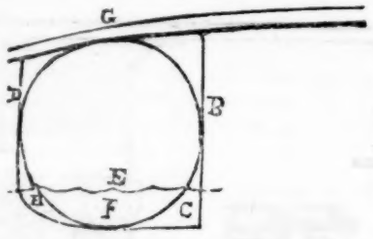
Sir,—Having for some months past observed in the papers notices of "Burden's Steamboat," and of the very great velocity with which she is expected to move, I was induced to institute a comparison between his construction and one which I invented, and have secured in the patent office at Washington; and the result is, that a boat built on my plan must move with greater velocity.

If I can establish the fact—which is the object of this communication—that a boat constructed on my plan, of equal length, and of as much weight as his, possessing a form calculated to move with less obstruction from the water, and to draw considerably less water than his, it must be manifest that my plan is superior, and must supersede his.

Before entering upon a comparison of the two plans, it is necessary that I should give you an idea of the form of my boat. You have only to imagine a boat *extremely long*, *very narrow*, with a flat bottom, similar to river steamboats of the present day, and *very sharp*, with *fine tapering extremities*, with the stem and stern posts in a curvilinear shape, and both inclined in opposite directions, as in common vessels, but at a very acute angle with the horizon. You have now only to conceive this boat split into equal parts, longitudinally, from stem to stern, down through the keel, and the two halves placed at any desired distance from each other in parallel lines, but joined above water by timbers and deck in the most substantial manner, and you have my plan.

I will now proceed to prove the superiority of such a construction over Burden's boat.

The *lines* of the figure marked A, present an end view of one of my twins cut *across* and *entirely through* at the centre, thereby showing the shape of the timbers or model of the twins, at the centre; the side timber, B, being 8 feet long, that is, that portion of it contained between the side timbers B and D. Within this figure is inscribed a circle of 8 feet diameter, representing an end view of one of Burden's twins, severed across at the centre also.



Now, for a clearer perception of the buoyant properties of the two plans, we will suppose that the above circle represents the circumference of a cylinder 100 feet long, which does not have tapering, pointed extremities, like Burden's, but whose ends are of the same diameter as the centre, viz. 8 feet. We will also suppose the lines of the above figure to represent the depth and width of a fabric 100 feet long also, whose ends shall have the same dimensions (8 by 8) and not pointed, as after my plan.

Bearing in mind these forms, we will assume that the weight of 200 tons are requisite to immerse the circular figure 2½ feet in the water, the surface of which is represented by the water line E.

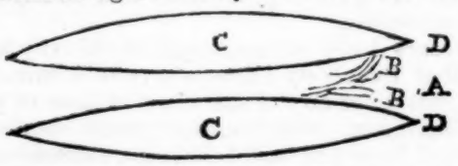
Now, a simple inspection of the two figures will suffice to show that 200 tons could immerse the black lined figure hardly more than half that depth, because, besides immersing an area equal to the segment F, it would have to immerse also the two areas G and H, which, together, are equal to ⅓, and a little more, of the area F.

The limits of the paper forbid entering into an exact mathematical calculation respecting the draft of each construction, but the foregoing figure and explanations must convince you that my plan is superior in respect of draft, and we know that the less the draft the greater the velocity, other things being equal.

Now, in regard to "other things," they are *not* equal—the inequality being in my favor. For, with respect to the heads of my boat, I can model them in a manner superior to that of Burden's, for dividing and gliding over the water.

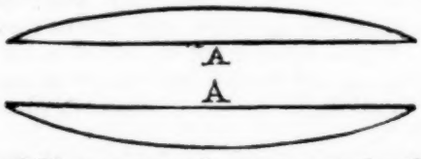
To perceive another advantage which my mode possesses over his, we will imagine that his deck is removed, and that we, being in the air over his boat, look down upon it. It will

of course present the following appearance—the twins being 16 feet apart at the centre C C.

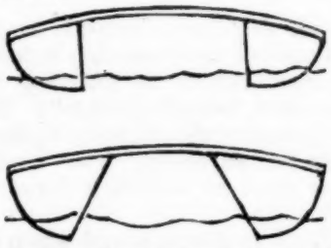


I am told that about $\frac{1}{11}$th of their length is above and clear of the water at each extremity. Therefore, supposing them to be in motion towards A, we at once perceive that a volume of water at B B, about 21 or 22 feet wide, (the distance between D and D being exact 24 feet,) must of necessity be compressed to a width of 16 feet in its passage at C C, and that the greater the power with which you urge the boat, the greater will be the accumulation of water at B B.

My boat viewed from above would present the following appearance:



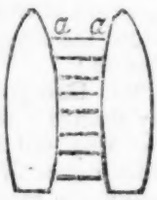
from which you can of course perceive that the water in its passage between the twins can meet with *no* obstruction. The two inner sides at A A can be either perpendicular to the surface or inclined toward each other, shown thus by end views:



In either mode the water passes without obstruction. For certain reasons it is thought that inclined inner sides, as in the under figure, are preferable.

My other advantages are, that I can use the holds of my twins, which Burden cannot do with his; and that I can construct a much stronger fabric, capable of withstanding a heavy sea, which cannot be said of his.

It is well known that twin boats have long been in use before Mr. Burden or I ever thought of our plans. The principal point of superiority in mine over all others is, that I dispense with those great obstacles to rapid motion, the *inside bows*, represented by *a a* in the annexed view of a common New-York ferry-boat.



They (the two inside bows) although as sharp as the two outside bows, it can be seen at a glance, impede a boat's progress vastly more than the two latter.

The keels of my twin boats being of a curvilinear shape, allow the boat every facility required in steering and turning. Respectfully,

CHARLES HARRIS.

P. S.—I will add that, with a rough model 5 feet long, the whole of which, with all her spars and sails, weighed not 10 pounds, having hoisted her sails on the river in a good sailing breeze, I could hardly catch her in a large boat, steered by myself, and propelled by a large sail and two stout oarsmen. She beat me when I used the sail only. If a little model would do this, what must be the velocity of a large vessel built upon this plan?

N. B.—The cuts intended to illustrate and explain the inventor's plans are so indifferently executed, that but an imperfect idea can be gained from them.

LANSINGBURGH, March 5, 1834.

To the Editor of the Mechanics' Magazine:

SIR,—I wish, through the medium of the Mechanics' Magazine, to offer a few remarks on a communication which appeared in the "Evening Star for the country," of February 28, signed by a Mr. Charles Harris, announcing his discovery of a plan of a boat which is to put Mr. Burden's boat at least into the back ground, if not into oblivion; of which wonderful discovery, Mr. H. has had the prudence to avail himself and heirs, by securing it at the patent office. I should not think proper to notice the above communication, were it not that there are persons possessing both the means and the disposition to patronize valuable improvements, but who have not sufficient acquaintance with mechanical science to enable them to judge with certainty as to the comparative merits of new inventions, until tested by experience. Such persons are liable to be imposed upon by plausible appearances, or to withhold that patronage from real merit which they would cheerfully give, if they knew where and when to bestow it.

With respect to Mr. H.'s plan of a boat, I have no disposition to question the sincerity of his belief, either as to the value of the discovery, or that he is the "*true and original inventor;*" but I wish to inform him, for his future benefit, and for the benefit of others, that he is as completely mistaken in one point as in the other; and he could not be more so in either.

With respect to his claim as the inventor, I would inform Mr. H. that Mr. Simon Fairman, now residing in this village, built a boat at Middletown, in Connecticut, in the year 1817, in the months of July and August, in all respects precisely on the plan which Mr. H. now claims as his. This boat, or model of a *steamboat*, was 35 feet long, and as Mr. H. very naturally describes it, was a boat split in two, lengthwise, through the middle, and the two approximate or inner sides were straight and parallel.

As it was not large enough, and indeed not intended for steam, he put in a wheel and prepared it to move by human power. It was exhibited at Middletown for some time, and he then, in the month of September, went down the river with it, and round to New-London, where it excited considerable notice.

The speed, however, was not equal to his

expectations. Upon strict examination, he found that the water in the straight passage being thrown back by the wheel, left a hollow towards the stern, which caused backwater. He then took out his wheel and built a false swell of considerable thickness on each of the two straight sides, and the result was a gain in speed, with the same power, from four to six miles per hour. After running it with passengers, a number of trips, between N. London and Norwich, he sold it for $300 to a gentleman who carried it to Demarara. So much for the originality of Mr. H.'s invention.

As to the superiority of strength his plan possesses over Mr. Burden's boat, the best way to decide the point is to make a strong iron bound barrel of good oak staves, and fill it with some heavy substance, say pork, for instance; and take the same kind of staves and make a square box to hold the same quantity, and bind it with the same weight of iron, and see which will endure the most violence without injury: or, what will amount to the same thing, prove that angles are stronger than arches.

In the advantage which Mr. H. calculates to gain over Mr. B.'s boat by the straight passage of the water through the centre and consequent removal of the angle of resistance in meeting the water, he will thereby add just as much to the angle, and of course to the resistance on the outsides.

I am not the advocate nor the eulogist of Mr. Burden. I am scarcely known to the gentleman, or he to me. But I should be sorry to see any gentleman deterred from encouraging Mr. B. and perhaps injuring himself thereby, and I should be equally sorry to see Mr. H. throw away his money, or that of any one else, under the mistaken idea that his plan is superior. He is certainly entitled to the satisfaction of trying the experiment, and I shall enjoy the satisfaction of having warned him of its inutility. ARCHIMEDES.

Mr. Rutter's New Mode of Generating Heat—Successful Application to Gas Works. [From the London Mechanics' Magazine.]

SIR,—I beg you will not impute my long-continued silence to any other cause than incessant occupation during the last three months. I am not indifferent to the opinions of some of your correspondents, nor am I insensible to the kind wishes which have of late been expressed towards me, in reference to my process for generating heat.

To dislodge prejudice is no easy task, especially when it has become venerable by age, or respected through the influence of great names. Truth will eventually prevail; but its progress bears no analogy to that of the glowing meteor. May it not rather be compared to the twilight of the morning, that melts imperceptibly into the dawn of perfect day?

It was once the fashion, I believe, to devise ingenious theories, and then endeavor to make practice conform to those theories. This seems to have been beginning at the wrong end. A more natural process is now adopted,—a process in which experiment supplies the materials for constructing theories and illustrating first principles.

Arrears have accumulated so heavily upon me that I scarcely know where to commence. If I pass over any of the observations of your correspondents which appear justly entitled to notice, I hope you will believe the omissions are unintentional. Until Colonel Macerone (No. 529, page 453,) republished his letter of November 25, 1826, I was wholly unacquainted with his views respecting the use of liquid fuel. I do not remember that I had ever seen, or heard, or read, any thing relating to the subject until the summer of 1832, and then my information extended no further than that coal tar was employed in heating gas retorts.

Mr. Cheverton (No. 531, page 28,) first made me acquainted with the name of Morey, whose 'American water burner' was very kindly and opportunely described (No. 533, page 52,) by Mr. W. H. Weekes. That experiment was new to me. I have since tried it, but, I must confess, without any perception of its being rendered "applicable, in many cases, in place of a furnace."

Mr. Cheverton implies a doubt as to the possibility of decomposing water economically. He admits, however, that "our knowledge of light, heat, and combustion, is very obscure, and that experiment is our best guide in the absence of perfect theory." I fully agree with him.

Almost all that was formerly known about the conditions of decomposing water by heat, seems to have related to effecting it in close tubes or vessels, heated externally, the hydrogen being liberated through the agency of readily oxidizable surfaces. The conditions of my process are very different from those just mentioned.

That water is decomposed when employed in conjunction with certain carbonaceous fluids, may be doubted or denied. It is one thing to deny, another to disprove: seeing, with some persons, is not always co-existent with belief. But truth is unalterable, whatever differences of opinion may prevail respecting it. In physical science, fact and experiment constitute the only safe guides to theory. Theories may be constantly changing, or they may be altogether false: not so the laws which govern the elements of the material universe. We may be ignorant of those laws, but are they on that account the less uniform and invariable in their results?

Mr. George Bayley (No. 533, page 52,) has my thanks: were he to see my memoranda of last May and June, he would be amused at the coincidence of our views respecting the use of tanks for stowing tar and preserving the trim of vessels. Will Mr. Bayley permit me to set him right in a trifling matter? The specific gravity of coal tar is *greater* than that of water.

The communication of your Salisbury "correspondent" was almost a verbatim copy of my own printed statement. I have no intention of interposing between "Correspondent" and "An Old Gas-maker," neither of whom are known

to me, excepting in your pages; yet I may perhaps be permitted to say a few words to the latter, in reply to his remarks, No. 537, p. 125.

It never was intended to set forth that 17,100 cubic feet of gas could be obtained from a chaldron of Newcastle coal *only* through the agency of my heating process. It would be mere trifling to say that heat, externally applied to retorts, whatever may be the kind of fuel employed, could materially affect the internal process of distillation. All other conditions being the same, I produce an equal quantity of gas from a given quantity of coal, whether the fuel employed be coal, or coke, coal and coke combined, or coal in conjunction with fluid materials. I will not say that an equal amount of work may be done in the same time, and at the same cost with solid fuel, as with solid and liquid together. In the former case it is more difficult to preserve the same uniformity of temperature than in the latter; and it is impossible, by any ordinary means, to obtain an equal quantity (or intensity) of heat from equal weights of solid and of liquid fuel.

The most startling part of 'Correspondent's' (or rather my statement), in the view of 'An Old Gas-maker,' seems to be that which relates to the density of the gas. An increased quantity of gas, and at the same time "an increased specific gravity," is "so much at variance with the generally received opinions of the present day," that "An Old Gas-maker cannot but conclude that there must be some error in that part of the statement." I have great pleasure in assuring him that there is not the slighest error. During the week last past, I have made from 81 bushels of Newcastle coal (80 lbs. per bushel,) 40,590 cubic feet of purified gas = 18,036 feet per chaldron = 14,028 per ton. Average sp. gr. 0.535.

These workings may not be in accordance with "the received opinions of the old school." I cannot be answerable for that. Experiment, not opinion, has been my guide. I only exhibit facts as I find them. By experiment I would have it understood that I mean not a *mere* experiment, which may rather be viewed as an exception than adopted as a rule. The manipulations of the laboratory are very often at variance with those of the manufactory. All my experiments have been conducted in the ordinary course of practical workings, and the results have been verified in the course of successive days, and weeks, and months.

At a neighboring station my mode of working has been successfully adopted. Last March the maximum product per chaldron on that station was 13,000 feet. In October, from the same kind of coal, 16,500 feet were made.

It has been said that it is impossible to make 18,000 feet of gas from a chaldron of Newcastle coal, since it involves the absurdity of getting more out of the retorts than had been put into them. This is assertion without proof, and is as much deserving of credit as the received opinions, that quantity and increase of sp. gr. are always in a direct inverse ratio.

To comfort an Old Gas-maker, I can assure him that the retorts I am now heating, by means which he is pleased to designate 'a new combination of the fiery elements,' are in excellent condition—and the furnaces stand well. The same retorts did almost all the work of the station here, during the last winter.

I am not sure that I have yet attained to the maximum product of gas from a given quantity of coal. After making 18,000 feet per chaldron, I find there are sufficient materials to make probably 4 or 5,000 feet more gas. At present I have no means of appropriating these materials for gas-making economically. With the same retorts, and the same apparatus that I am now working, I required, in Nov. 1832, to produce 46,580 feet of gas, 136 bushels of coal. In Nov. 1833, I have made an equal quantity of gas from 92½ bushels of the same kind of coal.

A question in which every practical gas-maker should feel interested, is, whether it be best to keep up a large fire principally for the purpose of making tar and coke, and ammoniacal liquor, or whether that fire may not be more profitably employed in generating gas?

It would extend this paper beyond all reasonable limits, were I now to enter more fully upon details. It would, moreover, be anticipating the proposed publication, in a different form, of my views and experiments connected with this subject. 'An Old Gas-maker' will, I hope, exercise a little patience; and if I appear to him somewhat slow in my movements, let him remember that I am neither an *old* gas-maker, nor a gas-maker by *profession*. All I do in that, or any other department of science, is during the few interstices of leisure that occur amongst more important engagements.

Will Mr. E. Walker, No. 536, p. 107, be so good as to inform the readers of the Mechanics' Magazine, by what means he has ascertained that coal gas, when used in close apartments, is a *deadly poison?* Is it absolutely necessary, when this gas is so used, that it should be permitted to escape into the room? What are the properties of the respective products of combustion resulting from purified coal gas and *long ten* tallow candles?

That I am deeply interested in these questions, Mr. Walker will perceive, when I tell him that I use gas in my bedroom instead of rush candles. I have done it because gas is cheaper than candles, and much more convenient.

J. O. N. Rutter.

Lymington, Nov. 27, 1833.

Mr. Rutter's New Mode of Generating Heat.

[From the London Mechanics' Magazine.]

Sir,—Mr. Rutter says, with truth, that his invention is never likely to be used where fuel is cheap, for according to his statement, although tar is but three-pence per gallon, it is still dearer than coal, the account standing thus:

4 cwt. 0 qr. 20 lbs. of coke at 25s. per ton,	5s. 2¼d.
27½ gallons of tar at 3d.....	6s. 10 d.
	12s. 0 ¼d.

And coal may be had at from 7s. to 12s. per ton.

Besides, as the demand for tar increases, so

must it increase in value, and thereby operate still further against its general adoption. Then, as regards originality, without wishing to detract one iota from the merit of Mr. Rutter, I think it but right to say that, at the gas-works here, tar has been consumed for some years past, not for the sake of economy, but merely to get rid of it, and, as I take it, in a manner somewhat analogous to that proposed by Mr. Rutter, viz.: over a certain measure of coke are thrown two cans of water, and one of tar; but from the effluvia arising from its combustion, this admixture cannot be used.

In commenting on the new mode of generating heat, Mr. G. Bayley, page 51, has been hurried, by an inconsiderate admiration, to wander far from the truth, and that, principally, through confounding weight for bulk. Want of "stowage" can only be understood to imply want of room, and a chaldron of coal and a chaldron of coke must occupy equal spaces; but as the former weighs 28, and the latter only 18 cwt., "stowage" cannot possibly be found in the same vessel for an equal weight of each. The comparison of volume and weight will therefore stand thus—

	cwt.	qr.	lbs.
A chaldron of coke	18	0	0
110 gallons of tar	10	3	6
	28	3	6

Which, multiplied by 3, will be equal to 86 cwt. 1 qr. 18 lbs. of coal.

Taking the specific gravity of coke and tar as equal, it gives 1·599 chaldron of the compound, and 3·084 chaldron of coals, or the effective quantities rather less than two to one, instead of three to one, as stated by Mr. Bayley.

As regards stowage, water of course need not be taken into account; but in estimating the comparative cost, 15 or 16 cwt. of water being required for every ton of coke, some allowance ought to be made on account of the labor, &c. necessary to remove it from alongside into the reservoir for supplying the fire. I will take leave of the subject at present, with noticing two minor errors: first, Mr. Bayley states the fluid required is withal of less specific gravity than water; while Mr. Rutter states the tar to be 11 lbs., and water is but 10 lbs. per gallon. Again, in stating the comparative economy, he says, 1½ gallons of tar at 1½d. per gallon, is a 1½d but this is no doubt a press error. Yours, respectfully, TREBOR VALENTINE.

Application of Mr. Rutter's New Mode of Generating Heat to Steam Vessels. [From the London Mechanics' Magazine.]

SIR,—My remarks, in No. 533, page 32, appear to have excited the displeasure of your talented correspondent, Trebor Valentine, on account of my "inconsiderate admiration" of Mr. Rutter's new method of generating heat, which, he says, had led me to wander far from the truth. This is a grave charge, and were it not made by a person in a mask, I might be induced to take it in high dudgeon.

How far I am justly liable to the charge of "inconsiderate admiration" of the process, is known only to myself. Not having a copy of my former letter at hand, I am unable to refer to the precise terms I then employed; but I have a distinct recollection that my "inconsiderate admiration" was chiefly referable to three points, viz., the saving of stowage, weight, and expense. It so happens that I have had some experience in building and navigating steam-vessels, and may, therefore, without presumption, lay claim to so much practical knowledge as to be qualified to express an opinion upon the desirableness of Mr. Rutter's method of generating heat.

I have often felt the want of convenient stowage for coals, notwithstanding there were many vacant spaces low down in the vessel, each containing a few cubic feet, in which small tanks for coal-tar might have been advantageously placed, but which were either unsuitable or inconvenient for the stowage of coal. The trifling weight of coke, compared with an equal bulk of coal, renders it much more convenient for stowage on board a steamer than coal, as it can be safely and advantageously stowed where it would be highly imprudent to stow coal, on account of its greater weight. Mr. Rutter's plan allows of the division of the fuel into small masses, so that, by due arrangement, it may be stowed in such a way as to lower the centre of gravity, and thus increase the stability of the vessel. Whether I have bestowed "inconsiderate admiration" on Mr. Rutter's plan, in this respect, I leave to you and your readers to decide.

With regard to the diminution of weight, I took Mr. Rutter's data, and, on reference to my former communication, think that it will be found that the difference is 3 to 1 in favor of Mr. Rutter's plan. Even on Trebor Valentine's own shewing, it is 2 to 1, which is sufficient advantage in point of weight to secure the "inconsiderate admiration" of persons like myself, who have found ourselves greatly impeded in our progress by the great weight of the coals we were compelled to take on board to supply the engines. But our "inconsiderate admiration" goes yet farther on this head, because the fluid admits of being stowed away in tanks, fitted to the vacant spaces near the bottom of the vessel, so as to bring the centre of gravity lower down than it usually is in steam-vessels worked with coals in the ordinary manner. This alteration in the centre of gravity would greatly increase the stability of a steamer, and lessen the labor of the captain and crew in trimming her upright when heeled either way, by the shifting of the passengers, or by the pressure of the sails. It is also very desirable to keep the vessel in as nearly the same trim as possible, that the paddle-wheels may act with their maximum effect. Mr. Rutter's plan offers the advantage of being able to dispense with half the weight we are now obliged to carry, and thus offers another attraction to call forth the "inconsiderate admiration" of the captains and engineers of steamboats.

It appears that I have made a blunder as to the specific gravity of the tar, and I stand corrected. I had no intention to misrepresent the fact, but having, as I thought, observed that

coal tar floated on salt water, I considered its specific gravity to be rather less.

I took the cost of the tar at 1½d. per gallon. I formerly purchased it at one penny per gallon, and not knowing its present price in large quantities, I thought that I had made sufficient allowance by stating it at three halfpence; and your correspondent, in No. 543, page 211, fully bears me out in my estimate being rather above than below the mark. Why Trebor Valentine should charge me with having wandered far from the truth, I cannot conceive, seeing that, for the purpose of convicting me of an error, he has at least doubled the cost of the ingredients in Mr. Rutter's plan for generating heat. Surely his "inconsiderate" prejudice, or something else, has led him far from the truth here, in order to decry a plan which, to say the least, is worthy of our approbation, for having suggested a method of applying to practical purposes a mode of generating heat which has hitherto been confined to the laboratory of the experimental chemist.

My calculation of the expense was made, as may be seen, upon the data furnished by Mr. Rutter, as to the quantities of fuel required to produce a certain effect. If he has erred in the proportions, my results are erroneous; but from the knowledge which I possessed of the cost of the articles, my impression was, and still is, that he had rather overrated the cost than otherwise.

If the cost of generating heat, upon Mr. Rutter's plan, should prove to be as great as by the present method, yet its saving of stowage and weight is sufficient to call forth the "inconsiderate admiration" of every one who has the charge of navigating a sea-going steamboat.

Trebor Valentine appears to have fallen into a very common error, viz. that if a certain weight must be carried on board a steam-vessel, it is of no consequence where it is put, whether on the deck or low down in the vessel's hold. Every nautical man knows that, with the same weight, a vessel may be either put into a good trim or be rendered utterly unseaworthy, according to the manner in which the weight is distributed, with regard to the centres of gravity and floatation.

It is a source of gratification to find that my views of the utility of the tanks for stowing tar coincide with those of Mr. Rutter; and I shall be glad to find that his plan is in actual operation on board some sea-going steamers; and I hope that you will not fail to give your readers the earliest information, with ample details of the results. To none will the information be more interesting than myself, for, although now unconnected with maritime affairs, I do not cease to feel a lively interest in every thing that relates to navigation and naval architecture. I am, yours, &c. GEORGE BAYLEY.

STEAM ENGINES.—The French Academy of Sciences have awarded a gold medal to M. Galy Cazalah, a professor in the Royal College at Versailles, for a discovery which, it is said, will give perfect security against the bursting of steam engine boilers.

INFLUENCE OF COLOR ON THE ABSORPTION OF HEAT AND OF ODOROUS PRINCIPLES.—On the 20th of June, 1833, a paper was read before the Royal Society, "On the Influence of Color on Heat and Odors," by James Stark, M. D., of Edinburgh; of which the following is an abstract.

The author observes, that the only experiments on record relating to the modifying effect of different colors on the absorption of heat from solar light are those of Franklin and Sir H. Davy. In order to investigate this subject, the author employed pieces of wool, silk, and cotton, which were wrapped round the bulb of a thermometer placed in a glass tube; the tube was then plunged into boiling water, and the time which elapsed during the rise of the thermometer from one given point to another was accurately noted. Other experiments were also made with an air-thermometer, of which the bulb was coated with various colored materials, and heat thrown on the ball by means of polished tin reflectors from an Argand burner. The results accord very nearly with those of Franklin and of Davy; the absorbing power with regard to different colors being nearly uniformly in the order of black, brown, green, red, yellow, and white. The author next investigates the differences which occur in the radiation of heat by differently colored substances; a subject on which he is not aware that any experiments have ever been made previously to his own. The mode of ascertaining the amount of radiation was generally the converse of that by which the absorption of heat had been determined: namely, by exposing the colored substances, in contact with a thermometer, to cooling instead of heating processes. The general result of all his experiments was, that the loss of caloric by radiation follows exactly the same order, with regard to the color of the radiating surface, as its absorption. In the second part of his paper the author gives an account of a course of experiments which he made with a view to discover the influence of color on the absorption of odorous effluvia, and more especially in the case of the absorption of the fumes of camphor and assafœtida by woollen cloth of different colors. Black cloth was always found to be possessed of the greatest absorbing powers, and white of the least; red cloth being intermediate between them. Cottons and silks gave, on trial, precisely the same results, which were further confirmed by the different weights acquired by these substances from the deposition of camphor upon them.—[Proceedings of the Royal Society.]

MIGRATION OF FISHES AND BIRDS.—"I fear I am not entomologist enough to follow the life of the May-fly, but I shall willingly have my attention directed to its habits. Indeed, I have often regretted that sportsmen were not fonder of zoology; they have so many opportunities, which other persons do not possess, of illustrating the origin and qualities of some of the most curious forms of animated nature; the causes and character of the migrations of ani-

mals; their relations to each other, and their place and order in the general scheme of the universe. It has always appeared to me, that the two great sources of change of place of animals was the providing of food for themselves, and resting-places and food for their young. The great supposed migrations of herrings from the poles to the temperate zone have appeared to me to be only the approach of successive shoals from deep to shallow water, for the purpose of spawning. The migrations of salmon and trout are evidently for the purpose of depositing their ova, or of finding food after they have spawned. Swallows and bee-eaters decidedly pursue flies over half a continent; the scolopax or snipe tribe, in like manner, search for worms and larvæ,—flying from those countries where either frost or dryness prevents them from boring,—making generally small flights at a time, and resting on their travels where they find food. And a journey from England to Africa is no more for an animal that can fly, with the wind, one hundred miles in an hour, than a journey for a Londoner to his seat in a distant province. And the migrations of smaller fishes or birds always occasion the migration of larger ones, that prey on them. Thus, the seal follows the salmon, in summer, to the mouths of rivers; the hake follows the herring and pilchard; hawks are seen in great quantities, in the month of May, coming into the east of Europe, after quails and landrails; and locusts are followed by numerous birds, that, fortunately for the agriculturist, make them their prey."—[Sir H. Davy's Salmonia.]

Opposition of Ignorance to the Use of Printing.—In the 'Typographical Antiquities' of Ames and Herbert, it is stated that the first book printed on paper manufactured in England came out in 1495 or 1496, from the press of Winkin de Worde. Shakspeare—whose chronology is not to be trusted—makes Jack Cade, in the reign of Henry VI., (who was deposed in 1461,) thus accuse Lord Sands: "Whereas, before, our forefathers had no other books but the *score* and the *tally*, thou hast caused printing to be used, and, contrary to the king, his crown, and dignity, thou hast built a paper-mill." The insurrection of Jack Cade was ostensibly for the redress of grievances amongst the people. Shakspeare fixes the complaint of Cade against printing and paper-making some ten or twenty years earlier than the introduction of printing amongst us; but he could not have better pointed out the ignorance of popular violence,—and all violence is the result of ignorance. The best instruments for producing good government, and equal laws for all men, have been the paper-mill and the printing press; and exactly in proportion as the knowledge which they embody has been diffused, have we advanced, not only in our social arrangements, but in every other manifestation of a presperous and well ordered community. Whatever remains to be accomplished will go hand-in-hand with the continued diffusion of knowledge.

Removal of a Steeple.—The Genoa Gazette contains an account of the removal of a church steeple entire, at Crescentino, in Piedmont, from one point to another, at several yards distance, where it was placed on a new foundation. The master mason was so confident of success that he made his son remain in the steeple ringing the bell during the operation.

Gas in the Railway Carriages.—We understand that measures are in progress for the introduction of portable gas for the lighting of the railway carriages. One carriage has been already furnished with this illuminating principle.—[Manchester Advertiser.]

Sir John Herschell has sailed on his astronomical mission to the Cape of Good Hope. He is expected to be absent about three years. He went out in the Catherine Stuart private ship, which has also on board Major-General Sir B. D'Urban and staff.

Observations on the Prevailing Currents of the Ocean and their Causes. [From the United Service Journal.]

There are few branches of science, connected with the phenomena of the surface of the globe, which have hitherto received less consideration than those oceanic currents which every-where prevail more or less in the great body of the waters; and it seems surprising, that in a country situated like our own, and so intimately connected with the element which forms our rampart, as well as the great medium of our wealth and greatness, so little has yet been done to trace this continued circulation to its true and proper source.

Much expectation on this subject was lately excited by the announcement of Major Rennell's work "On the Currents of the Atlantic;" and it was but natural to expect, from the pen of so able and experienced a writer, some elucidation of this hitherto obscure subject. The expectations entertained as to this posthumous work have in many practical points been fully answered; the *existence* and *effects* of many important currents have been explained in a manner that cannot fail to be highly useful to every practical man. But in tracing the *cause* of this mysterious movement, Major Rennell seems merely to have followed the usual track that had before been taken by all former writers on the subject, and has thus been led to attribute *to the winds*, effects which owe their real origin to the main cause of these very winds themselves.

It has long been known, that the prevailing currents, both in the air and in the waters, have a regular set within the tropics,

from east to west; and as atmospheric currents in these latitudes, under the name of the *Trade Winds*, have been justly attributed to the rotatory motion of the earth on its axis, it has been incautiously adopted as a principle, that the currents of the ocean arise from the action of the prevailing currents of wind, both in the tropics and in other parts of the earth.

"The winds," says Major Rennell, "are, with very few exceptions, to be regarded as the prime movers of the currents of the ocean; and of this agency, the trade winds and monsoons have by far the greatest share, not only in operating on the larger half of the whole extent of the circumambient ocean, but by possessing greater power, by their constancy and elevation, to generate and perpetuate currents; and although the monsoons change half yearly, yet the interval during which they continue to blow in each direction is long enough to produce effects *nearly similar to the constant trade winds;*" that is, although the winds do not always blow from east to west, but are, during one half of the year, *north-easterly*, and for the other half, *south-easterly*, yet the currents in the open ocean, within the tropics, are *constant*, from east to west, *and thus do not follow the direction of the winds from which they originate.* "The winds, then," concludes Major Rennell, "operating incessantly on the *surface* of the ocean, cause, in the first instance, a gentle but general motion *to leeward*, (as is proved by ships being always to leeward of their reckoning in the trade winds;) and the waters so put in motion form by accumulation streams of currents."—[Rennell on the Currents of the Atlantic, p. 6.]

Setting out, then, upon this principle, in his account of the existing currents of the ocean, as far as they are at present known, it cannot excite surprise, especially if this theory of the origin of the currents can be proved to be erroneous, if many facts are stated in the work of Major Rennell which are utterly at variance with the theory itself; and the consequence naturally is, that, however distinct and instructive the information may be with respect to the individual currents, and the best mode of combatting their influence, we rise from the perusal of the work more than ever uncertain as to the true *cause* of those remarkable streams which are known, in numerous instances, to run in the very face of the steady and prevailing winds which are here stated to be the occasion of them.

In these observations upon Major Rennell's work, we must not be understood, however, as detracting in any way from the highly useful tendency of it in a practical point of view, for which it was chiefly intended; but we beg to offer a few remarks as an attempt to elucidate this obscure but interesting subject, which, like other questions relating to an extended system, must first be viewed on a great and general scale, before we can safely venture to account for the minor portions of it, which come within our more immediate and personal observation.

It appears strange, that while the aerial currents of the atmospheric fluids within the tropics have been so long attributed to their proper cause, it should never have occurred that *the same cause* might probably have *the same effect* upon the aqueous fluids which cover so large a portion of the globe, and that the currents of the ocean might thus be mainly attributed, like the trade winds, to the rotatory motion of the earth upon its axis. The more powerful and constant of the currents of the tropics having a general tendency from east to west, might be supposed likely to suggest this idea. But if suggested, and put to the proof by actual observation *on a limited scale*, it is probable that the theory would be rejected as inconsistent with the facts, for while the trade winds are found to be in a great degree constant, like the cause which produces them, and only varying a few points to the north or south, according to the season, and the position of the earth with regard to the sun, the streams of the ocean are found to set in various directions, and frequently in *opposition* to the supposed cause; we could not, therefore, feel surprised if some other cause was immediately sought for.

In order, however, to set this point in a proper light, we have only to examine with attention the effects produced by a rapid and rocky descent on the small scale of a river or brook. We here find the general tendency of the stream taking, *as a whole*, a decided course, (say from east to west;) but if we confine our view to the minor parts of this stream, and watch the movements of any small floating substance, as it follows the various eddies and countercurrents occasioned by the rocky impediments in the bed of the river, we shall with difficulty bring ourselves to believe that the general tendency of the whole stream is from east to west, as we frequently find the floating bodies taking a direction from *west to east*, and, at some particular points, even from north to south. This is taking a limited view of what ought to be considered on a wider scale, and may serve as an illustration of what actually takes place when we form a theory for the

whole currents of the ocean, by merely observing some particular portions of it.

What takes place in a fluid on a small scale will assuredly occur also on a larger, as both are subjected to the same general laws; and because the trade winds are not so subject to opposition, and consequently to eddies and counter-currents, as the equatorial streams of the ocean, we are not, therefore, to conclude that both fluids are not originally set in motion by the very same cause, for it is obvious that, though their general tendency may be (as indeed it really is) *from east to west*, the numerous interruptions opposed to a regular movement in that exact direction may often occasion an extensive re-action in a direction to all appearance opposite to it.

Let us for a moment suppose the earth to be a body *at rest*, or at least without rotation on an axis; and let us further suppose no dry lands to exist above the surface of the waters, with which latter the sphere would thus be entirely covered. Let us also in idea remove its atmospheric envelope, that all friction or pressure may be removed between the two fluids of air and water: what, then, could we expect to find under such an arrangement? We could not look for any circulation in the watery covering, under such circumstances. Every thing would remain in perfect repose; and unless the waters were preserved in purity by some principle not now in existence, they would soon become corrupted and unsuitable to the nourishment of organic life. But let us now suppose a sudden impulse of rotation to be given to the sphere with its fluid covering; and let us consider what would be the effect of the rapid rotatory movement upon the circumambient waters. If a plate or other shallow vessel containing water be impelled in any direction horizontally, the fluid, participating but little in the impulse, *is left behind* on the spot whence the movement began; it cannot keep pace with the motion of the solid. In the same manner the globe would revolve upon its axis, while the superficial waters would remain nearly stationary, and would have all the appearance of moving in opposite directions, *seeming* to transport floating bodies from east to west, while, in point of fact, the earth *was passing them* from west to east. Thus we perceive that, in the supposed case which we have now put, the steady movement of the solid ball would be imperceptible, while floating bodies on the surface of the water would visibly become more distant in an opposite direction. This apparent movement would naturally be greatest in the equatorial regions, being the outer rim of the revolving wheel; while towards the axis the waters would be little, if at all, affected by the rotation.

Let us now, for a moment, suppose our globe to be surrounded with its atmosphere, or envelope of fluids of a different nature. Without rotatory movement in the solid, there could be no semblance of regular movement in this aerial fluid, and we could therefore have no trade winds. Other partial winds there would be, it is true, occasioned by heat acting on the elasticity of the air, and by a constant succession of expansion and contraction arising from various degrees of temperature. But if we suppose, as before, a revolving and rapid motion to be given to the sphere thus surrounded with its airy envelope, a similarity of cause would immediately occasion a similarity of effect. The *trade winds* would be produced in the equatorial regions, while the circulation of the atmosphere in the more temperate and frigid latitudes would be carried on by the changes of temperature in the same manner, or nearly so, as if there had been no rotatory motion at all.

The effect of the trade winds, and, by analogy, of the oceanic currents, may be simply illustrated by the example of a well-mounted horseman in a calm day. While he remains still, not a breath of air blows. He moves slowly, but produces little effect in deranging the quiet of the atmosphere. The more rapid his course, however, the more violent will be *the current of air* which *seems* to blow in his face whichever way he goes; and even in the case of a moderate breeze, he may "outstrip the wind," and make it seem to blow in an opposite direction.

It must be obvious, then, that the effects of the revolving motion of our globe must be the same, both upon *the fluids of the air*, and upon *the fluids of the ocean*, and consequently, that the regular *trade winds*, and the regular *equatorial currents*, proceed each separately from this cause, and would equally exist even in the absence of the other. But it may be urged, that the trade-winds are much more constant to their course than the equatorial currents, and it therefore seems difficult to imagine that they can both proceed from the same cause. The reply to this objection is extremely simple, when we look a little deeper into the nature and circumstances of the two fluids. The atmospheric fluid is above the surface of the solid, and is but slightly deranged by the asperities and interruptions it may have to encounter, in the form of the islands, continents, or mountains of the earth. Some derangement actually does take place, however, from these

causes, but it bears no comparison to the counter-currents and eddies which are found in the ocean, arising from the numerous and insurmountable obstacles which are thrown in the way of the regular equatorial streams. In the supposed case, which was before put merely for illustration, we considered the globe to be entirely covered with the waters. Such is not, however, the reality now, although this preternatural effect has certainly existed, on one most memorable occasion, the evident traces of which attest the fact on every part of the surface of the globe. Such is not, however, the usual state of things; on the contrary, the ocean occupies about two-thirds of the whole surface, while the remainder is broken into a thousand dispersed fragments, each opposing its solid form, as the sphere revolves, to the regularity of the oceanic movements. If the smooth and polished wheel of the turner be made to revolve in water, the movement, however rapid, produces little or no commotion in the fluid; but let the polished wheel be changed for one having a toothed or unequal edge, and we shall instantly perceive a very opposite effect. The effects of the paddles of the steamboat on smooth and tranquil waters will also bring this subject home to the mind of every one. We cannot then look for the same regularity of movement in the equatorial currents of the ocean that is perceptible in the equatorial currents of the atmosphere.

In considering the origin of the currents of the ocean, it must be kept in mind that they proceed from two distinct causes, and thus exhibit one of the most wonderful and provident effects to be seen in the order of the works of the Creator. Water and air, if left stagnant, soon become corrupt and unwholesome; and it is evidently a wise provision of the Almighty, which has furnished the laws by which a constant circulation and movement are kept up in both. In the case of the atmosphere, the circulation occasioned by the *winds* takes place, partly by means of the revolutions of the earth on its axis, and partly by the expansive nature of air when affected by the heat of the sun. The lower beds of the atmosphere are elevated into the higher regions by heat; and other portions of the fluid, rushing in to fill up the vacuum, occasion streams of wind of various degrees of force. The seasons of the year, and the duration of the effects of summer and winter in various latitudes, also occasion similar currents of air more or less durable, according to circumstances. But in the case of the currents of the ocean, there are but two causes from which constant currents can primarily arise: one from the rotatory motion of the earth, from *west to east*, which causes an *apparent* current from *east to west* in the open seas near the equator; the other cause arises from the inclined position of the earth with regard to the sun, by which a greater *evaporation* takes place from the waters of the sea within the tropics, than in the more temperate and frigid zones; and on the other hand, a proportioned *condensation* of this vapor (in the form of rain, dew, and snow,) takes place in the latter regions, greatly superior in quantity to what falls, during the whole year, in the former. These effects of temperature are so vast, when viewed upon the scale of the whole earth, that the balance of the ocean would be deranged by them, thus *losing* water in one region and *regaining* it in two others. This want of equilibrium is, however, obviated by constant currents in the ocean, from the poles towards the tropics.* In figure 1 of the following plate, (where the outer line denotes a supposed boundary to the atmosphere,) we see the vapors rising from the equatorial regions, and passing towards the poles, where they return to their parent deep, in the form of dew, rain, and snow. Thus restored to the ocean, they flow towards the tropics, and there chime in with the prevailing currents, in their course to the westward. In the central part of the same figure an idea may be formed of the effect of an intervening continent, in opposing its solid form to the fluids through which it is rapidly and constantly passing, with greater velocity than those fluids can possibly follow it. At 1, the equatorial current meets an opposing cape, which divides it into two parts: one flows pretty freely from the north-west, being kept, however, in its place by the north polar currents pressing towards it. It meets another projection at 7, still farther to the north; and after passing it, the stream is *forced* into its more natural position near the equator, and proceeds in its westerly course, after forming a great counter-current or eddy in the sheltered gulf at 6, where navigators would fall in, for days toge-

* It is probable, perhaps even certain, that *heat* has also a very considerable influence in keeping up the movement and circulation of the waters, but it is not likely that currents of great extent are set in motion by this cause. Water, like air, expands by heat, and contracts by a certain degree of cold, not, however, so low as the freezing point, for at that temperature ice is formed, and the formation of ice is always accompanied by violent expansion, so great, indeed, as to burst the strongest vessels, and to cause explosions like cannon, in the lofty glaciers of Alpine regions.

As warm water rises above the colder, (except in the extreme case of ice, which always floats,) and as currents and counter-currents are always acting *horizontally*, and then intermixing the fluids from the poles and from the tropics, it is obvious that an interchange must also be constantly going on *vertically*, in the waters of the ocean, and thus completing the circulation of which the great superficial currents, already described, are the leading cause.

ther, with what would appear, if viewed on a small scale, totally opposed to the theory now under explanation. Returning to the Cape at 1, we find the other half of the northern equatorial stream proceeding to the south-west, where it fills the deep gulf, or sea, at 3, and keeps up the waters there at a high level, on a principle which will immediately be explained. It cannot, however, make its escape in a body or current from this gulf, but, being confined by the southern division of the equatorial stream, a variety of eddies on a considerable scale are produced at 2. It is unnecessary to explain the figure further, by proceeding to the southern hemisphere, where similar effects are produced by nearly similar causes at the points 4 and 5; we may therefore proceed to explain upon what principle the level of the sea in the gulf at 3 is kept up at a higher level than the same surface in the bay at 6, an effect which is known to exist in several remarkable instances on the globe, and which, according to the theory, ought to exist in every situation similarly situated.

By fair analogy, we find that, in this, as in other parts of nature, what takes place on a small scale may also be looked for on a larger. Proceeding then upon this principle, and considering minutely the rapid and rocky course of a brook or river, we find that, so long as the water flows over a smooth and equal bed, the depth and surface of the stream are in all places alike, as in figure 2 of the plate. But when, on the other hand, a fixed and solid opposition is encountered, in the form of a projecting rock, derangement in the level instantly takes place, to a degree proportioned to the bulk of the opposing object. An accumulation or rise in the water takes place on the upper side, until the current finds a vent at one or both extremities, and without this vent, the accumulation increases until the water flows over the top, when the difference of level above and below the object is at once apparent (see fig. 3). But supposing the impediment to be small, in proportion to the size of the stream, still, in every case, a change of level must be the consequence; and the recovery of tranquillity is only completed at some distance below the object, where it, at length, falls again into the general inclination. Beneath or behind this opposing rock, then, there is a sheltered nook, upon which the stream can only act in the form of an eddy; and in such nooks buoyant objects are often kept, as it were, imprisoned by the force of the stream on each side, and floating round in one continual circle. These eddies of the smaller rivers are equally well known to fish and fishers, as both are there sure to find their wished-for prey. Now, all these effects are to be expected on the great scale of the ocean current, as well as in the smaller instance of an inland brook. The *streamward* side of these mighty rivers will always be found on a higher level than the *eddy side;* and consequently the inland gulf at 3, (fig. 1,) ought to be considerably higher than the waters in the bay at 6, which remains sheltered from the powerful action of the current. Thus the level of the Red Sea, which is filled and kept up by the action of a powerful stream across the Indian Ocean, was found, by the French engineers, to be so considerably higher than that of the Mediterranean, that much difficulty and expense would have been incurred in the canal which was once projected across the isthmus of Suez, in order to facilitate the communication with India by this route. A second instance of this effect no doubt exists in the gulf of Mexico, compared with the level of the north Pacific on the western coast of Mexico; but the actual difference of level has not yet been ascertained. A remarkable instance, however, of this difference of level, obviously arising also from the above cause, has been kindly communicated to us by Sir H. Douglas, who was then governor of New Brunswick, where it was found that, in a proposed canal intended to have been cut from the top of the Bay of Fundy to Bay Verte, in the Gulf of St. Lawrence, (a distance by land of only fourteen miles,) the difference of level of the two seas was no less than sixty-three feet,* the rise of the tide in Bay Verte being only seven feet, while that in Cumberland Bay, of the Bay of Fundy, exposed to the full force of the Gulf stream, was seventy feet. The Bay of Fundy is kept at this high level in consequence of the projecting peninsula of Nova Scotia impeding the current which rushes along that coast towards the north, and which, from the bend of the coast towards the north-east, is carried in that direction, leaving the gulf of St. Lawrence as a sheltered eddy or nook.

We may now proceed to a cursory view of the whole existing system of the currents, as far as the observations of navigators have made us acquainted with them; but in the rapid sketch which is alone consistent with the limits of a paper of this description, it would be impossible, and even injudicious, to be led from the general outline into any notice of the innumerable minor currents of which seamen have frequently made men-

* Surveyed by Mr. Francis Hall, and reported upon by Mr. Telford.

Fig. 1.

Theory of the General System of the Currents both of aqueous and aerial Fluids.

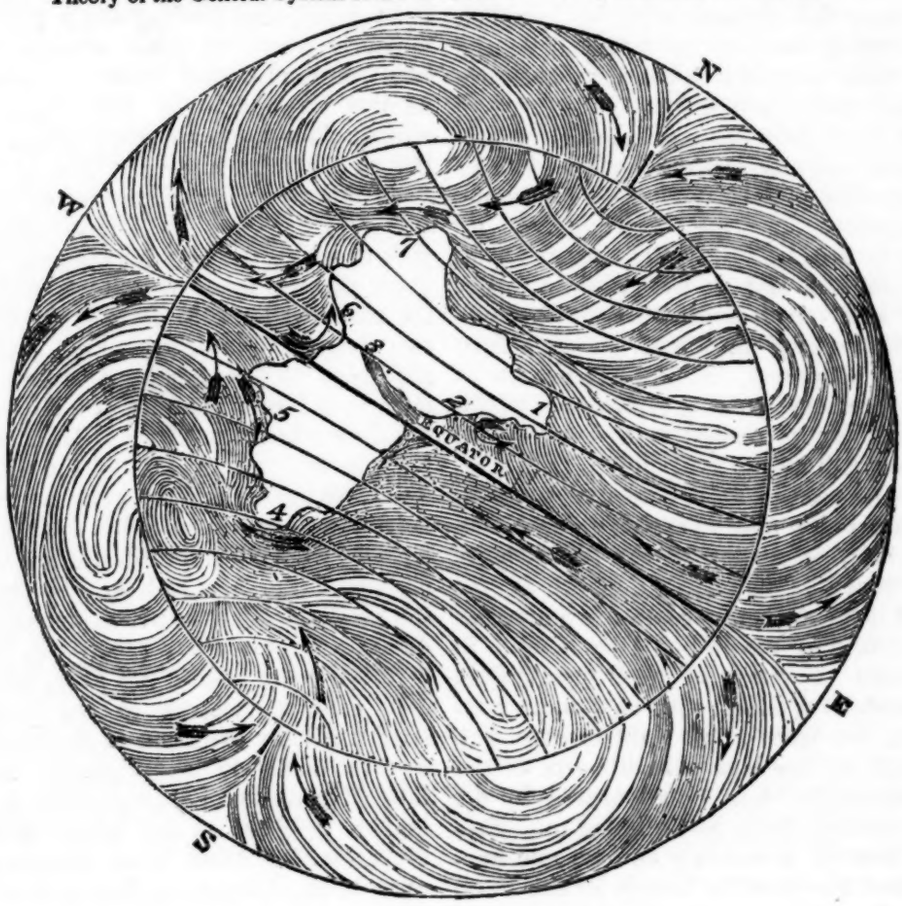


Fig. 2.

Unopposed Current.

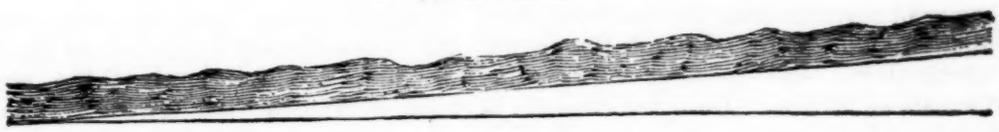


Fig. 3.

The Consequence of Opposition.

tion, and which may often be looked upon as eddies and counter-currents, produced by the main body of the stream,* and being occasioned by a variety of changing circumstances, may not again be found in the same exact position.

As it is necessary in this circuitous course to start from some particular point, which may be considered as it were a commencement of the circle, we may adopt as the most proper the western line of the continent of America, whereby the circle is more nearly broken, from pole to pole, than by any other of the dry lands of the earth. Setting out then from this point, and viewing more especially the equatorial line of currents, we

* Major Rennell's work on the currents is accompanied by a laborious and valuable volume of charts, which, if any objection could be made to them, might be considered so minute as to produce confusion. It appears that the courses of the minor eddies have been laid down wherever any naval authority could be produced for their existence, although it is more than probable that a large proportion of them may not again be found in the same position by future navigators.

enter the immense expanse of the North and South Pacific, where every account that has touched upon the currents tends to establish the fact of their westerly course; and as the force of these currents must there be more steady and equal than on any other part of the globe, from their being unopposed by any thing more important than clusters of small islands, we should not expect them to assume that dangerous and impetuous power by which they are frequently distinguished in the Chinese sea, and in the Atlantic. Mr. Mariner, and other navigators, have given us some interesting proofs of the existence of westerly currents, in the adventures of parties of natives, passing from one island to another, being carried to a distance of many hundred miles, and being found on islands from whence they were utterly hopeless of ever being able to regain their native shores. Of this portion of the globe, however, it must be admitted that we as yet know but little with regard to the currents. But if we find in the Indian Ocean, and in the Atlantic, a series of well established facts in support of the system now under consideration, we have a full right to extend it, by analogy, to the less visited parts of the globe, especially when corroborated by the few but striking facts just alluded to. Proceeding then in a westerly course, and having reached the western bounds of the Pacific, with the Chinese islands and shores on the one hand, and the continent of New-Holland on the other, we hear of a succession of powerful currents from the eastward, forcing their devious courses through the crowded archipelago, and pointing towards the east coasts of Africa. Here the currents of both sides of the equator, being confined in a much smaller space than in the Pacific, and being forced by the form of the land out of that position which is naturally given them by the rotatory motion of the earth, become more violent, and consequently more obvious. In their efforts to retain their position north of the equator, they act with great force against the shores of the seas of Bengal and of Arabia, occasioning in the former the well known and formidable surf of Madras. Finding no vent in a northerly direction, the united stream is forced to the southward, along the east coast of Africa, and if left at liberty, it would follow the southerly impetus thus given to it, and flow into the southern ocean. In this, however, it is opposed by the south polar currents, and it therefore no sooner arrives at the Cape of Good Hope than it doubles that point, in the well-known Lagullas stream, and, running in a north-westerly direction, hastens to regain its natural position on each side of the equator. The force of this current off the Cape is so great, that nothing but a prevalence of westerly winds at some seasons could enable outward-bound ships to make head against it; and even with these favorable winds, ships are constantly found driven to the westward in the very face of the wind.

In following out the course of the equatorial stream across the Atlantic, we find it in part crossing the equator obliquely, and this great moving mass of waters, striking upon the eastern point of Brazil, is divided into two streams, one driven to the southward by the form of the coast of South America, until it is forced round Cape Horn, as it had before doubled the Cape of Good Hope, and joins in with the waters of the Pacific; the other, taking a north-westerly course towards the Caribbean sea and the Gulf of Mexico, passing with considerable force amongst the islands of the West Indies. Having reached the Gulf of Mexico, which opens its extended arms, as it were, to receive it, the current is there brought to a full stop, being precluded from advancing to the westward, or northward, by the form of the lands, and the waters being in consequence *accumulated* into a higher level than perhaps in any other known position of the whole globe. This elevation has often been *supposed*, and has even been shown to be demonstrably certain, without, however, any good reason having even been assigned for the phenomenon. We here, therefore, find a natural, and even necessary cause, upon the same principle as has been already explained by fig. 3 of the plate. The high level of the sea in the Gulf of Mexico cannot, however, pass a certain boundary, and the swell of waters at length finds relief by the only possible, though tortuous course, that is left open for its issue. The stream then rushes with a violence proportioned to its late confinement, round the south point of East Florida, and here, taking the name of the Gulf Stream, it proceeds to the northward, along the coasts of the United States to Newfoundland, where it encounters the Great Bank, and becomes again divided, one portion continuing towards the north and east by Iceland and the coast of Greenland, until again stopped by the north polar currents; and the other, bending to the east and south, is terminated in an immense vortex in the centre of the north Atlantic, where it accumulates on the surface prodigious quantities of the *fucus natans*, or Gulf-weed, which is known to flourish in the warm waters of the Gulf, and to be carried by the stream into the Atlantic, and there covers the surface for hundreds of

miles, together with floating timber and other bodies, washed out by the rivers of America. In this great eddy, then, the famous Gulf Stream may be said to terminate; but not so the other portion of the current which had passed on towards the north: when met by the north polar currents from the arctic seas, it is headed back towards the south, along the coast of Norway, and into the North Sea. We here feel its effects upon our own coasts, especially of the north of Scotland, and of Ireland, where floating substances from southern latitudes are frequently found. A minor branch passes through our channel, and rejoins the greater stream across the Bay of Biscay; and the whole at length becomes blended once more in the equatorial current off Cape Verde and the coast of Africa.

In an interesting work which has recently appeared—the Narrative of Capt. Owen's Voyages for the Survey of the Coasts of Africa—we have a distinct proof of the great obscurity which still overshadows the subject of the currents. In the observations on the results which have been gained by this long, interesting, and most fatal expedition, we find the greater part of the subject connected with the currents summed up in the following passages at the end of the work.

"As in the foregoing narrative but few observations have been introduced respecting the currents, and as it is a subject of much speculation and interest, at least to those connected with navigation, the following remarks from Capt. Owen's Journal may be considered worthy of publicity.

"It is a well known fact, as regards the African seas, *that there is a perennial current which sets into the Atlantic Ocean*, round the entire southern extreme of that continent; this current varies in its velocity in different situations, and at different periods, from five miles to one mile an hour. Some writers have supposed that, with reference to the Great Ocean, the Atlantic may be considered as a kind of mediterranean sea, the evaporation from which, together with winter frosts to the northward, must be supplied from the Southern Ocean, in like manner as the Mediterranean is fed from the Atlantic; *and this hypothesis is borne out* by the strong perennial currents about the shores of Cape Horn, and through the islands in its vicinity. But it is remarkable that these currents never appear to extend more than *twenty leagues beyond the common deep-sea soundings*, while their velocity is much decreased when near the shore; from which it may be understood that the depth is much diminished, and the stream broken by projections of bank and sand.

"Ships are frequently carried to the westward, quite round the Cape of Good Hope, *even against the strongest north-west gales*, by this current."

Capt. Owen then proceeds to state the dangerous nature of the short though *high* waves produced by the currents and wind being in opposition, and the most effectual course by which the danger may be avoided. It is quite clear that every thing here stated is strictly in accordance with the theory here advanced. He bears witness to the *constancy* of the current from east to west; and in other parts of his work, when treating of the east coasts of Africa, and those of Madagascar, he mentions the rapid nature of the currents *passing down from the northward towards the Cape*, by which, in one instance, the Leven, in making the point of Mombas, was driven so far to the southward, that it took her *six days* to regain what she had lost by the failure of the wind for about *three hours*.—[Vol. II. p. 150.]

It is known also that, off the Cape, ships have been driven to the westward, at the rate of sixty or seventy miles per day, *even against* a strong westerly wind.

The only part of Capt. Owen's statement which in any degree stands opposed to what is now advanced, is the allusion to the constant currents at Cape Horn. These are not *stated* to run to the eastward, or *into*, instead of *out of* the Atlantic, but that fact is implied by the theory of *evaporation* from the Atlantic, which is counter-balanced by *entering* currents at both capes. This is opposed by the general reports of the navigation of Cape Horn; it is opposed also, most distinctly, by the much better attested facts of currents *out of* the South Atlantic towards the north. For if evaporation took place on so great a scale as to produce *entering* currents at the two great capes, we must admit that an *entering* current should also flow from the colder latitudes of the north, which is not the case. As to the fact of the current at the Cape being little felt *close in shore*, and gradually diminishing in force as it extends to the open ocean, a hundred miles or more from land, it is in every way consistent with the whole theory of inland rivers. In the case of a projecting bank or rock in a river, the actual point of contact is exposed to great violence, but every other point of the stream exhibits the phenomena described by Capt. Owen off the Cape. Under the most projecting rock or point, comparatively smooth water is generally found close to the side; while the main

body of the stream drives past with a distinct and rippling outline, diminishing in force, however, as it spreads out into the expanding pool below.*

We have thus passed in review the great and leading course of this wonderful and most admirable system by which the waters of the ocean are kept in that continued movement so necessary to their purity, and by which, also, it is highly probable that many important ends are effected, in regard to the amelioration of the climates of various parts of the earth. The land and sea breezes of the hotter climates are now well known, and also their causes. We may naturally suppose this wholesome interchange to be powerfully affected by streams of current from the cooler latitudes; and we also may be assured that the heated waters of the Gulf Stream must carry along with them into the Frozen Ocean a degree of warmth which cannot but materially affect the rigidity of those latitudes. Even in our own country, we are well aware, from continued experience, of the mild effects of a westerly wind. We have no particular warmth to look for from the *lands* to the westward of us; on the contrary, the winters of Labrador and of Canada are well known to be unusually severe. But when we find that a vast reservoir of heated water, and consequently of *warm vapors*, exists in the Atlantic, we can no longer find a difficulty in naturally accounting for the mild and humid effects of our westerly winds, which, even in winter, produce on Ireland and the west coast of Britain the verdant growth of a milder season.

It is scarcely necessary, in conclusion, again to revert to the theory of the winds being the prime movers of the currents; for besides the arguments already adduced, by which we trust it has been shown that ocean currents could not but exist, even if there were no winds whatever, we have only to examine the numerous instances mentioned even by Major Rennell, of ships being drifted far to *windward*, in the very teeth, not of transient breezes alone, but of settled and heavy gales. "One ship," says he, "was carried 10° of longitude (*equal to* 570 *miles*) to the westward, between Cape Verde and the Cape of Good Hope, and yet had been subjected to the south-east trade wind. Another was driven 220 miles, between the Canaries and the coast of Brazil. Another in the equatorial current, in June and July, was set 297 miles to the westward, *in five following days*, between 3° north and 4½° south, and yet had entered the south-east trade wind." Such, and numerous other instances, well known to all seamen, are sufficient to show that the currents must be set in motion by some much more powerful and less *superficial* cause than the mere *friction* of the winds, however fixed or severe. That the winds agitate the *surface* of the waters no one will attempt to question; but that this agitation can extend to the vast depths at which the law of fluids above explained must operate, we have not the slightest reason to suppose. Major Rennell brings forward, in proof of his theory, the well known fact, that the surface of a canal, or of a lake, is always higher at the *leeward* than at the *windward* side. This fact is at once admitted, but it is one of very small effect, and merely *superficial*, being occasioned by *waves*, and instantly subsiding with these waves. But in order to prove the point, it must be shown, that in a straight canal of several miles in length, with a strong breeze right on end, the force of the winds, near the middle of the distance, (where they must have acquired their full force,) can affect an object of no great weight *at the bottom of the canal, and at a depth of four or five feet.* If this effect takes place in canals, or in large inland lakes, such as those of North America, and also at considerable depths, the theory might be supposed to derive some support from it. But this is not the case; and in inland lakes, of whatever extent, although the surface may be raised on the leeward side, in violent winds, objects deposited at a few feet of depth lie perfectly secure and unmoved.

The winds would not, therefore, effect the end for which the great circulation of the waters of the ocean is obviously intended; and any theory of the currents, which is mainly founded on so false a ground, however ably it may be treated, cannot but mislead the mind, and in many instances prove injurious, not only in a scientific, but also in a practical point of view.

* Major Rennell gives many interesting instances of bottles and other bodies carried by the currents. In one case a bottle was thrown overboard from the Osprey, of Glasgow, on the 17th of January, 1822, in 6° 13′ south latitude, and 15° 35′ west longitude, and it was found on the 27th of July of the same year, in Mayard Bay, in the island of Trinidad.

In another case, still more remarkable, a bottle was thrown from the American ship Lady Montague, on the 15th of October, 1820, two leagues north-east of the island of Ascension, and was picked up *on the west coast of Guernsey*, the 6th of August, 1821, and notice of it sent to the Admiralty. It is certain that this bottle must have passed, in ten months, over the whole course of the Gulf Stream, and from thence be carried (probably by the coasts of Iceland) into the North sea, and through the English Channel. We cannot, however, decide from this, or almost any instance of floating bodies, as to the *rapidity* of the current, for we cannot tell how long it may have been detained at various points, nor how long it may have remained on the spot where it was eventually discovered.

History of Chemistry. [Continued from page 108.]

Of Mercury.—Mercury, like some other metals, appears to have received its name from the planet, with which it was compared by the Persians on account of its nature, which was supposed to approach to that of gold, as this planet is nearest to the sun, and has been known since the most remote ages of antiquity. From a comparison of its qualities with silver, it was long ago termed *quicksilver, hydrargyrum.* In the species of hieroglyphics that were formerly employed for representing bodies, mercury was represented by the combined signs of the sun and moon, or of gold and silver, linked together, and supported upon a cross.

The alchemists have labored much upon this metal. They considered it as very much resembling gold and silver, and differing but very little from them; they imagined that it wanted but very little to become either the one or the other, and they always hoped to discover the means of transmuting it into these metals. Some of them have even affirmed, that they had succeeded in effecting this transmutation. These adepts agree with each other, that it is much more easy to convert it into silver than into gold. According to them, in order to convert it into silver, nothing more is required than to fix it. It was, therefore, in this fixation of mercury that they made all the art of their opus magnum, all the marvellous part of their science, to consist; this was the grand object of their attention, and the scope of all their wishes. All these pretensions, however, are not supported by a single well-attested fact; and the more we advance in the study of the properties of mercury, the more differences we find between it and the metals to which it has been supposed to approach the nearest.

The most celebrated philosophers, and the most able chemists, have all successively occupied themselves with this metal: they have endeavored to ascertain all its properties with more or less precision; and the use which has been made of it since the end of the last century, or since the time of Boyle, in the construction of a great number of philosophical instruments, has afforded frequent occasion for investigating and examining its different characters. It is in this manner that its weight, its phosphorescence, its dilatibility, its volatility, its alterability, its mobility, &c. have been successively ascertained.

The application of the pneumatic chemistry has connected together all the known facts relative to the chemical properties of mercury; it has given rise to the discovery of a considerable number of new ones; it has elucidated a great number of facts which before could not be explained; it has drawn from oblivion several which were neglected, or in a manner abandoned; it has dissipated all the obscurity, ambiguity, or uncertainty, that remained in the enunciation of its properties; it has conducted chemists to several important discoveries: such as the mutual differences between the greater part of the metallic or mercurial salts; the comparative state of the different oxides of mercury; the action of each oxide upon this metal or its oxides; the formation of several triple salts; the cause of the energy and causticity of the mercurial salts or oxides; the spontaneous reduction of these oxides; their decompositions by some of the metals; the nature and characters of several precipitates; the different states of some of its solutions; the extinction of mercury in a number of substances, which had always been considered as a simple division, but which, in reality, is a true oxidation. Mercury being one of the most useful of metallic substances for medical purposes, for the arts, and for all those branches of knowledge which extend our views of nature, we shall give a full detail of its properties.

Mercury is always fluid when in a pure state, and is one of the most brilliant and shining of all known metals. When its surface is sufficiently clear, it forms a very fine mirror. Its color is as beautiful as that of silver, with which it has always been compared. After platina and gold, it is considered as the heaviest of all known bodies. Its specific gravity is 13·568, taking water at 1·000. Authors were formerly very particular in observing that all the most ponderous substances swam upon its surface, whilst gold alone sunk in it; at the present day we have to add to this, platina and tungsten.

Boerhaave asserted, in his Elements of Chemistry, that mercury could not be rendered solid by any degree of cold, though he admits a condensation of $\frac{1}{360}$ of its primitive volume; a circumstance which cannot take place in its real congelation. This assertion of Boerhaave, and other philosophers who have followed him, was proved to be false in the year 1759; in which year the academicians of Petersburg, availing themselves of an intense degree of natural cold, augmented it still further by a mixture of snow and fuming nitrous acid; the mercurial thermometer which they used descended to 213 degrees of De Lisle's scale, which corresponds with 46 below 0 of that of Fahrenheit. As the mercury did not descend any lower, but seemed stationary, the academicians broke the glass bulb of their instrument, in which they found congealed mercury, that formed a solid sub-

stance susceptible of being extended by the hammer. They thus discovered that mercury might become solid, and that it in this state possessed a certain degree of ductility. They remarked that, at every stroke of the hammer, the pressure, developing the caloric in the interior of the metal, fused it, and that it ran into globules.

This first experiment was, in some measure, nothing more than a hint to philosophers concerning a property unknown, and till then even denied, in mercury; it has since been often repeated, and of late it has become as easy and simple an experiment as most of those that are made in Chemistry. In the year 1772, Pallas caused mercury to congeal, at Krasnejark, by a natural cold of —55½ deg. of Fahrenheit's scale. It was observed that it then resembled soft tin; that it could be flattened; that it broke easily; and that its fragments, when brought into contact, were glued or soldered together, as happens in all other softened metals. However, it is evident that he did not obtain its real conversion into a solid, or complete concretion, as the mercury was still soft, and only in a state of semi-congelation. In the year 1775, Mr. Hutchins observed the same congelation at Albany Fort, and Mr. Bicker at Rotterdam, in 1776, at 56 deg. below 0. In the year 1783, the congelation of mercury was effected in England with a less degree of cold; and Mr. Cavendish has proved 31½ below 0 of Reaumur's, or 40 below Fahrenheit's thermometer, to be the real degree at which it takes place.

As a metal always fused, always liquid at the temperature of our climate, mercury constantly affects the form of perfect globules when it is divided. When inclosed in a glass phial or tube, its surface is convex, which depends upon the small attraction which it has for the glass; in fact, if we pour it into a vessel or tube of some metal with which it is able to combine, instead of remaining convex, its surface becomes concave. As this round, curved, and convex surface may give rise to some errors in barometrical observations, especially in those which are made with tubes of a fine bore, in which the elevation of the mercury ought to be an exact measure of the height of the places which we wish to determine, attempts have been made to obviate this source of error, by rendering the mercurial surface flat. Cassebois succeeded by boiling the mercury for a long time in the barometrical tubes; by which means a surface almost perfectly horizontal was obtained, especially in tubes of a wide bore.

The expansion of mercury by the action of fire has not yet been very accurately determined; it is known to be a very good conductor of caloric, on which account it appears very cold to the hand when immersed in it; and it is also owing to this conducting property that a red hot iron, when plunged into mercury, instantaneously loses its redness, which it would have retained for some time in the air, and even in water. Its expansion by heat proceeds in a very uniform manner; and it is on this account that it is employed in the construction of thermometers. When it is penetrated with a quantity of caloric, which has not yet been well ascertained, but which is estimated at 656 degrees of Fahrenheit's thermometer, the mercury swells, is reduced to the state of vapor, and volatilized. When this experiment is performed in the air, the mercury presently condenses into a white smoke, which is capable of producing very injurious effects upon animals. If it be performed in close vessels, in such a manner that the metal may speedily become fixed and liquified, this becomes a means of distillation, in which the habitudes of the volatile metal are the same as those of every other distilled liquid. In this operation, which is often employed for the purpose of purifying the mercury, it is customary to adapt to the neck of the retort of iron or stone ware which is used a tube of linen, the extremity of which is immersed in the water with which the receiver is filled. By means of this apparatus the mercury is speedily condensed into the liquid form, and collected entire under the water, from which it is afterwards separated by rubbing it with paper manufactured without size, drying it in a gentle heat, passing it through a skin, agitating it with very dry bread-crumbs, bran, and other desiccating means of a like nature. It is on account of this easy process of distillation that chemists have considered mercury as the most volatile of all metals.

Mercury is a very good conductor of electricity and galvanism. Its electrical property is probably the cause of the phosphorescence, and the considerably bright light which it emits when it is agitated in a vacuum. It has been discovered that this phosphorescence is an electrical phenomenon, which takes place only in consequence of the friction of the mercury against the sides of the tube, and that the mercury does not thereby suffer any sensible alteration.

Mercury is not altered by being kept under water. When exposed to the air, its surface is gradually tarnished, and covered with a black powder, owing to its combining with the oxygen of the atmosphere. But

this change goes on very slowly, unless the mercury be either heated or agitated: by shaking it, for instance, in a large bottle full of air. By either of these processes the metal is converted into an oxide; by the last, into a black oxide, and by the first, into a red colored oxide. This metal does not seem to be capable of combustion; at least, no method which has hitherto been tried to burn it has succeeded. It is the only metal which may not, by peculiar management, be made to *burn*.

Native mercury, which has been termed virgin mercury, is found in the form of liquid globules, which are very easily recognized by their brilliancy and liquidity. It is commonly found in tender and friable earths and stones, and frequently interposed between the fissures and the cavities of its own orbs, especially of its sulphuret. It is seldom perfectly pure, and frequently contains some other metal with which it is alloyed; but when it is sufficiently liquid, it is considered as pure, or really native. At Ydria, and in Spain, and America, it is collected in the cavities and clefts of the rocks, into which it filtrates from all sides. It is found liquid in argil at Almaden, and in the beds of chalk in Sicily. It is also found in the ores of silver and lead, and even mixed with the arsenious acid, or white arsenic.

Mercury does not combine with the simple incombustibles; but it combines with the greater number of metals. These combinations are known in chemistry by the name of *amalgams*.

The amalgam of gold is formed very readily, because there is a very strong affinity between the two metals. If a bit of gold be dipped into mercury, its surface, by combining with mercury, becomes as white as silver. The easiest way of forming this amalgam is to throw small pieces of red hot gold into mercury heated till it begins to smoke. The proportions of the ingredients are not determinable, because they combine in any proportion. This amalgam is of a silvery whiteness. By squeezing it through leather the excess of mercury may be separated, and a soft white amalgam obtained, which gradually becomes solid, and consists of about one part of mercury to two of gold. It melts at a moderate temperature; and in a heat below redness the mercury evaporates and leaves the gold in a state of purity. It is much used in gilding. The amalgam is spread upon the metal which is to be gilt; and then, by the application of a gentle and equal heat the mercury is driven off, and the gold left adhering to the metallic surface; this surface is then rubbed with a brass wire brush under water, and afterwards burnished.

Dr. Lewis attempted to form an amalgam of platinum, but succeeded only imperfectly, as was the case also with Scheffer. Morveau succeeded by means of heat. He fixed a small cylinder of platinum at the bottom of a tall glass vessel, and covered it with mercury. The vessel was then placed in a sand bath, and the mercury kept constantly boiling. The mercury gradually combined with the platinum; the weight of the cylinders was doubled, and it became brittle. When heated strongly, the mercury evaporated, and left the platinum partly oxidized. It is remarkable that the platinum, notwithstanding its superior specific gravity, always swam upon the surface of the mercury, so that Morveau was under the necessity of fixing it down.

There are few metallic substances that exceed mercury in utility. In physics, it is employed in its metallic form; in the construction of meteorological instruments, and a great number of machines in the arts, it is employed in the same form for gilding, silvering of glass mirrors, and in metallurgical operations; its solutions are used in dyeing.

In chemistry it is applied to a great variety of uses, all equally important. Besides the experiments in which it is employed for demonstrating the principal truths of this science, it has become of indispensible necessity for furnishing the vessels destined to collect, preserve, and combine many of the gases.

It is of equal importance for medicinal purposes.

Smoky Chimneys. By Comfort. To the Editor of the Mechanics' Magazine.

In the number of your periodical for January, there is an article headed "smoky chimneys," accompanied by rules for their cure, condensed from the works of Count Rumford.

It may be observed that the improvement in fire-places was proposed by Count Rumford with a view to economy in the article of fuel, and the suffusion through rooms of an increased quantity of heat from any given quantity of fuel. The Count, indeed, observes, that his plan of a fire-place will often act as a cure to smoky chimneys, but his chief object was economy in fuel, and his experiments had this as their chief, if not their sole object.

The result of his studies, scientific and operative, led to the conclusion, in his mind,

that four inches is the proper width for the throat of chimneys, and this will probably be found applicable to as many, or to more cases, than any other, which, as a general standard, could be adopted. It will not, however, be equally applicable to all cases, nor will any general rule, in this respect, apply universally.

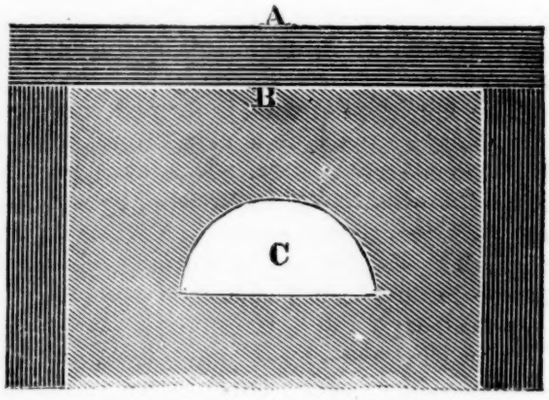
The proper width of the throat of a chimney is the least which will admit all the smoke, together with the quantity of rarified air necessary to aid its escape through the chimney. This must be regulated by circumstances, and chiefly by the material of the fuel. Anthracite coal, producing little smoke, would require a throat even narrower than four inches. Coal, of the quality of the Sydney, producing a large volume of smoke, might perhaps require a width of throat exceeding four inches.

The reasonableness of thus narrowing the throat of the chimney for the purpose of yielding an increased heat to the room, will, upon the least reflection, be sufficiently obvious. The smoke, if not impeded by some obstruction, will naturally ascend through the chimney; the heat of the fire, from its affinity to smoke, will ascend with it. Should the volume of air constantly rushing into the room find an over-easy passage through the chimney, the entire, or very nearly the entire, of the heat will escape with it; hence the fact, that persons sitting in a room, in presence of a large fire, often suffer from cold, and even in a degree greater than they would were there neither fire nor fire-place in the room.

A chimney may, however, be so narrow in some part of it, or throughout the whole, that it will not admit all the smoke, a part of which will, in such case, in search of a new channel of escape, make its way into the room. A similar effect will be produced when the chimney is so injudiciously constructed that the smoke cannot escape through it with the required ease and rapidity. A chimney may be so placed in relation to another chimney within the same building, as to cause it to smoke. These different causes of smoky chimneys may require very different remedies. Count Rumford's plan of narrowing the throat of a chimney will often effect a cure, but surely not universally. I propose herein to offer a remedy, which, although not proposed as an universal panacea, will, it is presumed, effect a cure in the majority of cases of smoky chimneys.

It is generally known that by lowering the mantle-piece the draft is increased, and the smoky chimney thus partially or fully cured; but it is also known that this mode will, by increasing the draft, lessen the quantity of heat in the room, and that warmth is in this way dispensed with, to avoid the annoyance of smoke. A plan which would yield the advantage of a lowered mantle without its disadvantages would be a desideratum. This is, perhaps, not to the full desirable extent practicable; it is certainly practicable to a considerable extent. It may be introduced in aid of the Count's plan, where that fails, as a remedy for smoky chimneys, or it may be adopted in cases where mere economy is not an object, or where it would be inconvenient to resort to the Count's plan.

On reference to the cut No. 2, in your Magazine of January, it will be seen, that in order to reduce the throat of the chimney, there is a false back. This is made of solid work, and is extended about six inches above the breast of the chimney, where the width of four inches is acquired, and is that part distinguished as the *throat*. We may suppose this false back to be in thickness equal to the length of a common brick, say eight inches. If made of the breadth of a brick, there would be a vacant space of four inches between the false and the real back. By turning an arch in the false back, or by an aperture of any other shape, there might be left an opening for the admission of smoke and air into the vacant space between the



A, chimney-piece—B, false back—C, aperture.

backs. This would produce all the effect of a lowered mantle, without producing all the inconvenience. The portion of inconvenience which it might produce would be entirely provided against by a metal casting fitted to the arch-way, and supplied with a door, to be closed or opened as occasion might require.

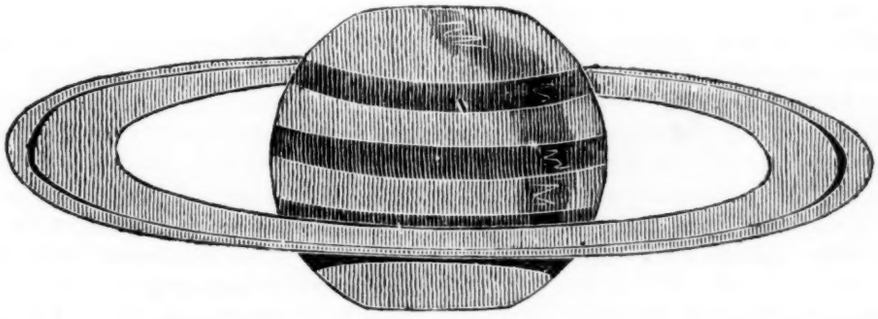
This contrivance would most probably relieve the occupants of houses, in nine cases out of ten, from the annoyance of smoky chimneys, and from the heavy charges of the chimney doctor. COMFORT.

History of Astronomy—its various Systems, &c. [Continued from page 103.]

Of Saturn.—The planet Saturn is 79,491 miles in diameter, and performs his revolution round the sun in 10,746 days, 19 hours, 16 minutes, at the distance of 903,690,197 English miles. His motion in his orbit is said to be 18,000 miles per hour; and the time he revolves on his axis 10 h. 16′ by some astronomers, and by others only 6 hours.

Saturn is distinguished from all the other planets by a large luminous ring surrounding his body, which was discovered by the celebrated Huygens, about the end of the 17th century. The same astronomer also discovered the fourth satellite, which attends this planet, and on that account is sometimes called the Huygenian satellite. The ring which surrounds Saturn appears double when seen through a good telescope, and is seen to cast a deep shadow on the planet. Dr. Herschel is of opinion that the ring has a motion round its axis; but this is doubted by Schroeter, and some other astronomers. Respecting the formation of this strange phenomenon, astronomers have been very different in their opinions, but the difficulty still remains as formidable as ever.

The annexed figure represents this planet as seen by Sir William Herschel, on various occasions, with his powerful telescope.



To Saturn the sun appears only one-ninetieth part of the size it does to the earth; and the light and heat which that planet receives from the sun are in the same proportion. But to compensate for the scantiness of light derived from the sun, Saturn has been observed to have no fewer than seven satellites revolving round him, besides the luminous ring that surrounds his body. The Huygenian, or fourth satellite, was the first discovered; the first, second, third, and fifth, were some years afterwards discovered by Cassini; and the sixth and seventh were discovered by Dr. Herschel, in the year 1789. These satellites are all so small, and at such a distance from the earth, that they cannot be seen, unless with very powerful telescopes.

The orbit of the planet Saturn was long considered as the boundary of the solar system, except the cometary orbits, which were always believed to stretch far beyond it. But by the discovery of the planet Georgium Sidus, this system has been extended far beyond the limits formerly assigned it.

Of Georgium Sidus.—A new planet was discovered by Dr. Herschel, on the 13th of March, 1781, and called by him Georgium Sidus, out of respect to his Majesty George III., but astronomers have given it the names of Herschel and Uranus. This planet is situated far beyond the orbit of Saturn, being at the immense distance of 1,822,568,000 miles from the sun. The time it requires to perform its revolution round that luminary is 83 years, 150 days, 18 hours. Its diameter is about 4½ times greater than that of the earth, or nearly 35,000 English miles. The distance of this planet from the sun being about double that of Saturn, can scarcely be discovered by the naked eye. However, when the sky is very clear, it may be perceived by a good eye, like a faint star of the fifth magnitude; but it cannot be readily distinguished from a fixed star with a telescope of a less magnifying power than 200. This planet is placed at so great a distance from the sun that it can receive but a very small portion of his light; however, this want is in some measure supplied by six satellites that revolve round it, all of which were discovered by Dr. Herschel. The periodic times of these satellites are as follows: The first is 5d. 21h. 25′; the second, 8d. 7h. 1′ 19″; the third, 10d. 23h. 4′; the fourth, 13d. 11h. 5′; the fifth, 38d. 1h. 49′; and the sixth, 107d. 16h. 40′. It is a remarkable circumstance that all these satellites move round the planet in a retrograde order, and that their orbits are nearly all in the same plane, almost perpendicular to the ecliptic.

NEW PLANETS.

Of Ceres.—The planet Ceres, which is

situated between the orbits of Mars and Jupiter, was discovered at Palermo, in Sicily, on the 1st of January, 1801, by M. Piazzi, an ingenious astronomer, who has since distinguished himself by his numerous observations. Ceres is of a ruddy color, but not deep, and appears about the size of a star of the eighth magnitude. It seems to be surrounded with a large dense atmosphere, and plainly exhibits a disc when examined by a telescope which magnifies about 200 times. Ceres performs her revolution round the sun in 4 years, 7 months, and 10 days, and her mean distance from that luminary is nearly 260,000,000 English miles. The eccentricity of her orbit is a little greater than that of Mercury, and its inclination to the ecliptic exceeds that of all the old planets. The magnitude of this planet is not yet well ascertained. Dr. Herschel makes her diameter only 160 miles, while Schroeter makes it 1624 miles. This great difference, says Schroeter, was occasioned by Herschel observing with his projection-micrometer at too great a distance from the eye, and measuring only the middle clear part of the nucleus.

Of Pallas.—The planet Pallas was discovered at Bremen, in Lower Saxony, on the 28th of March, 1802, by Dr. Olbers. It is situated between the orbits of Mars and Jupiter, and is nearly of the same magnitude with Ceres. It is of a less ruddy color than Ceres, and performs its revolution round the sun nearly in the same time. The atmosphere of this planet, according to Schroeter, is to that of Ceres in the proportion of two to three. It undergoes similar changes, but the light of the planet exhibits greater variations.

Of Juno.—The planet Juno, situated between the orbits of Mars and Jupiter, was discovered by M. Harding, at the Observatory of Lilienthal, near Bremen, on the evening of the 1st of September, 1804. While M. Harding was forming an atlas of all the stars which were near the orbits of Ceres and Pallas, he observed in the constellation Pisces a small star of the eighth magnitude, which was not mentioned by La Lande, in his *Histoire Celeste*, and being ignorant of its latitude and longitude, he put it down in his chart as nearly as he could estimate with his eye. Two days afterwards the star disappeared; but he perceived another that he had not seen before, resembling the first in size and color, and situated a little to the south-west of its place. He observed it again on the 5th of September, and finding that it had moved still farther to the south-west, he concluded that it was a planet. It is of a reddish color, and free from that faint whitish light that surrounds Pallas. Its diameter and mean distance from the sun are less than those of Pallas or Ceres.

Of Vesta.—This planet, which appears like a star of the sixth magnitude, of a dusky color, similar in appearance to Herschel, was discovered on the 29th of March, 1807, by Dr. Olbers, who gave it the name of Vesta. In a clear evening it may be seen by the naked eye. Its light is more intense, pure, and white, than any of the other three new planets. The time it takes to perform its annual revolution is 3 years, 66 days, 4 hours. Its diameter is stated at 238 miles, and its mean distance from the sun at 225,000,000 miles.

of comets.

Of all celestial bodies, comets have given rise to the greatest number of speculations. In the ages of ignorance and superstition, they were believed to be the harbingers of divine vengeance, and to portend great political and physical convulsions. The most ancient opinion respecting their nature was, that they were enormous meteors formed in the earth's atmosphere. Yet many of the ancients entertained opinions respecting them agreeing with some parts of the *modern* hypothesis respecting these bodies; for they believed that they were so far of the nature of planets, that they had their periodical times of appearing, and that when they were out of sight they were carried aloft to an immense distance from the earth, but again became visible when they descended into the lower regions of the air, when they were nearer to us. Modern astronomers are now generally agreed, that they have no light of their own, and appear luminous only by the light of the sun. They have no visible disc, and shine with a pale whitish light, accompanied with long transparent trains, or tails, proceeding from that side which is turned away from the sun. When a comet is viewed through a good telescope, it appears like a mass of vapors surrounding a dark nucleus of different degrees of opacity in different comets. As these bodies approach the sun their light becomes more brilliant, and after they reach their perihelion, often exceed any of the planets in lustre. Their tails are also observed to increase, both in length and brightness, as they approach the sun. The opinions of astronomers respecting these tails have been very different. Tycho Brahe, who was the first that gave the comets their true rank in the creation, supposed that the tail was occasioned by the rays of the sun passing through the nucleus of the comet, which he believed to be transparent. Kep-

ler thought that it was the atmosphere of the comet which was driven behind it by the force of the solar rays. Sir Isaac Newton maintained that the tail was a thin vapor, ascending by means of the sun's heat, as smoke does from the earth. Euler supposes that the tail is produced by the impulse of the solar rays driving off the atmosphere from the comet. Dr Hamilton, of Dublin, supposes them to be streams of electric matter.

In any of these opinions there is little to entitle it to preference above the others; and till multiplied observations shall have added to the imperfect knowledge which we at present possess of these bodies, it is perhaps better not to give a decided preference to any of them.

From a number of observations made by Sir Isaac Newton on the comet that appeared in the year 1680, he was enabled to discover the true motion of these bodies.

Dr. Halley, following the theory of Newton, set himself to collect all the observations which had been made on comets, and calculated the elements of 24 of them. By computations founded on these elements, he concluded that the comet of 1682 was the same that had appeared in the years 1456, 1531, and 1607; that it had a period of 75 or 76 years; and he ventured to predict, that it would appear again about the year 1758, which it actually did; therefore it may be expected to appear again in the year 1835.

When a comet makes its appearance, it is only for a very short period, seldom exceeding a few months, and sometimes only a few weeks. Instead of moving from *west* to *east*, like the planets in orbits making small angles with the ecliptic, they are observed to cross it at all angles. Their progress among the fixed stars is in general more rapid than that of the planets, and their change of apparent magnitude is much more remarkable. When a comet retires from the sun, its tail decreases and nearly resumes its first appearance. Those comets which never approach very near the sun have nothing but a coma or nebulosity round them during the whole time of their continuance in view.

The tail of a comet is always transparent, for the stars are often distinctly visible through it, and it has even been said, that on some occasions they have been seen through the nucleus or head. The length and form of the tail are very different. Sometimes it extends only a few degrees, at others it extends more than 90 degrees. In the great comet that appeared in the year 1680, the tail subtended an angle of 70°, and the tail of the one which appeared in 1618, an angle of 104°. The tail sometimes consists of diverging streams of light: that of the comet which appeared in the year 1744 consisted of six, all proceeding from the head, and all a little bent in the same direction. The tail of the beautiful comet which appeared in 1811 was composed of two diverging beams of faint light, slightly colored, which made an angle of 15° to 20°, and sometimes much more. Both of them were a little bent outward, and the space between them was comparatively obscure.

The apparent difference in the length and lustre of the tail of comets has given rise to a popular division of these singular bodies into three kinds, namely, *bearded*, *tailed*, and *hairy* comets; but this division rather relates to the several circumstances of the *same* comet, than to the phenomena of different ones. Thus when the comet is *east* of the sun, and moves *from* him, it is said to be *bearded*, because the light precedes it in the manner of a beard; when the comet is *west* of the sun, and sets after him, it is said to be *tailed*, because the train of light follows it in the manner of a tail; and when the sun and comet are diametrically opposite, the earth being between them, the train or tail is all hid behind the body of the comet, except the extremities, which being broader than the body of the comet, appear to surround it like a border of *hair*, and on this account it is called hairy. But there have been several comets observed, whose discs were as clear, round, and well defined, as that of Jupiter, without either tail, beard, or coma.

The magnitude of comets has been observed to be very different; many of them without their *coma* have appeared no larger than stars of the first magnitude; but some authors have given us accounts of others which appeared much greater: such was the one that appeared in the time of the emperor Nero, which, as Seneca relates, was not inferior, in apparent magnitude, to the sun himself. The comet which Hevelius observed in the year 1652 did not seem to be less than the moon, though it was deficient in splendor, for it had a pale, dim light, and appeared with a dismal aspect. Most comets have dense and dark atmospheres surrounding their bodies, which weaken the sun's rays that fall upon them; but within these appears the nucleus or solid body of the comet, which, when the sky is clear, will often give a more splendid light.

Respecting the nature of these singular and extraordinary bodies, philosophers and astronomers in all ages and countries have been very much divided in their opinions.

The vulgar have, however, invariably considered them as *evil omens*, and forerunners of war, pestilence, famine, &c.; and to adopt the language of an old poet,

"The blazing star was viewed—
Throat'ning the world with famine, plague, and war;
To princes death; to kingdoms many crosses;
To all estates inevitable losses;
To herdsmen rot; to ploughmen hapless seasons;
To sailors storms; to cities civil treasons."

The Chaldeans, who were eminent for their astronomical researches, were of opinion that comets were lasting bodies, which had stated revolutions as well as the planets, but in orbits considerably more extensive, on which account they are only visible while near the earth, but disappear again when they ascend into the higher regions. Pythagoras taught that comets were wandering stars, disappearing in the superior parts of their orbits, and becoming visible only in the lower parts of them. Some of the ancient philosophers supposed they were nothing else but a reflection of the beams from the sun or moon, and generated as a rainbow; others supposed they arose from vapors and exhalations. The illustrious Aristotle was of opinion they were meteors. Modern philosophers have been equally perplexed as their predecessors in accounting for the nature of these magnificent celestial appearances.

The eccentric but learned Paracelsus gravely affirmed that they were formed and composed by angels and spirits, to foretel some good or bad events. Kepler, the celebrated astronomer, asserted that comets were monsters, and generated in the celestial spaces by an animal faculty! The sentiments of Bodin, a learned French writer of the 16th century, were yet more absurd; for he maintained that comets are spirits which have lived upon the earth innumerable ages, and being at last arrived on the confines of death, celebrate their last triumph, or are called to the firmament like shining stars!

James Bernoulli, a celebrated Swiss philosopher, formed a rational conjecture relative to comets, in viewing them as the satellites of some very distant planets invisible on the earth on account of its distance, as were also the satellites, unless when in a certain part of their course. Tycho Brahe, the illustrious but unfortunate philosopher of Denmark, supported a true hypothesis on this subject. He averred that a comet had no sensible diurnal parallax, and therefore was not only far above the regions of our atmosphere, but much higher than the moon; that few have come so near the earth as to have any diurnal parallax, yet all comets have an annual parallax; the revolution of the earth in its orbit causes their apparent motion to be very different from what it would be if viewed from the sun, which demonstrates that they are much nearer than the fixed stars, which have no such parallax.

Descartes advanced another opinion, which is, that comets are only stars that were formerly fixed like the rest, but becoming gradually covered with *maculæ* or spots, and at length wholly deprived of their light, cannot keep their places, but are carried off by the vortices* of the circumjacent stars; and in proportion to the magnitude and solidity, moved in such a manner as to be brought nearer to the orb of Saturn; and thus coming within reach of the sun's light, are rendered visible.

The number of comets belonging to the solar system is said not to be less than 450; but the periods of not more than three of these are known. The velocity of these bodies, and their distance from the sun, when in the remotest part of their orbits, exceed all human comprehension. Sir Isaac Newton calculated the velocity of the comet of 1680, and found it to be 880,000 miles per hour, and its aphelion distance not less than 11,200,000,000 miles.

Respecting the use of these bodies, many conjectures have been formed. Mr. Whiston thought it probable that they were appointed by the Almighty as places of punishment for sinners after *death*, who would be alternately tormented with the most insupportable *heat* when nearest the sun, and in the opposite point with the greatest possible *cold*.

Sir Isaac Newton, amongst other purposes which he thinks they may be designed to serve, adds, "that for the conservation of the water and moisture of the planets, comets seem absolutely requisite, from whose condensed vapors and exhalations all the moisture which is spent in vegetation, and turned into dry earth, &c. may be supplied and recruited, for all vegetables grow and increase wholly from fluids; and again, as to their greatest part, by putrefaction into earth. Hence the quantity of dry earth must continually increase, and the moisture decrease, and be quite evaporated, if it did not receive a

* Descartes supposed that every thing in the universe was formed from very minute bodies called *atoms*, which had been floating in open space. To each atom he attributed a motion on its axis; and he also maintained, that there was a general motion of the whole universe round like a *vortex*, or whirlpool. In the centre of this vortex was the sun, with all the planets circulating round him at different distances; and that each star was also the centre of a general vortex round which its planets turned. Besides these general vortices, each planet had a vortex of its own, by which its satellites (if it had any) were whirled round, and any other body that came within its reach.

continual supply from *some* part or other of the universe;"—"and I suspect," adds this philosopher, "that the *spirit*, which makes the finest, subtilest, and best part of our air, and which is absolutely requisite for the life and being of all things, comes from the comets."

On Undulating Railways. By A CIVIL ENGINEER. To the Editor, &c.

SIR,—Soon after the perusal of the experiments made by Mr. Badnall on the Liverpool and Manchester Railroad, I drew up the following strictures upon them, which I send for publication, if you should think that they will contribute to the objects of your Journal.

A CIVIL ENGINEER.

MR. EDITOR,—You must pardon me--my patience is exhausted—I can no longer silently look on and see your respectable and useful paper countenancing an absurdity which nothing but the blindest species of infatuation (that of an inventor for his favorite project) can support after a moment's serious reflection, aided by a knowledge of the laws of nature. But I must pause --perhaps you have thought, from my abrupt beginning, that I was about to attack another Cataline: you are mistaken--I allude to Mr. Badnall's "undulating railway," and *especially to his late experiments on the Liverpool road*, which, it would seem, were seriously witnessed by Mr. Stephenson, the Engineer of that great work.

After reciting the experiments, the conclusions drawn from them are,—"That a locomotive engine can not only convey, on an *undulating way, double the load* which it is capable of conveying at *the same velocity* on a *level line*, but that it can accomplish this by the employment of *half its power*." [See page 28, Vol. iii.]

That Mr. Badnall should persuade himself of the truth of this most unphilosophical conclusion, is not a matter of much wonder; but that Mr. Stephenson, who is an eminent engineer, a man on whom the epithet 'great' has, on some occasions, been conferred, should sanction it, is to me a cause no less of surprise than regret.

I have declared his conclusions erroneous; it remains to sustain my position. With this view the following observations are premised:

It is a law of gravitation, that, if a body fall freely from a certain height to the earth, its velocity is *accelerated* at each instant of time during its fall: that, *cæteris paribus*, if the same body be projected upwards with the final velocity it had acquired by its fall, it will rise to the same height from which it fell, and in the same time, before its momentum will be destroyed by the same force which generated it, in its downward course.

Bodies moving on inclined planes follow the same general immutable law,—i. e., if a body be projected up an inclination with a certain initial velocity, it will advance up the plane until its momentum is exhausted by its gravitating tendency, when, if left free, it will return down the plane in a time equal to that consumed in its ascent, and at the end of its course will have acquired precisely the initial velocity of projection.

We hence perceive *that no more power or momentum can be generated by a body in falling down a plane than will carry it up an equal inclination in length and degree.*

These preliminaries made, we may remark that the first six experiments are of one class. We will abridge the 1st, 2d, and 7th, 8th experiments, which contain the substance of the whole set.

1st. "*Two* engines brought the train to a given point at the foot of the Sutton inclined plane, at which point they had attained a velocity of 19 miles per hour. One engine then left the train, and the other ascended with the load 575 yards in 116 seconds."

2d. "The power of the engine being reversed, the engine and load descended 575 yards in 84 seconds; the velocity attained at the foot of the plane being *far greater* than at the same point when *ascending*."

7th. "The two engines attained a velocity of 18 miles per hour at the foot of the ascent; one engine then left the train, and the other shut off her steam, when the whole train rose, by *momentum only*, 332 yards in 70 seconds."

8th. "The train descended (the engine working) 332 yards in 66 seconds."

In the first set of experiments there is an apparent inconsistency with the law of dynamics above given: that this is only specious, and not real, as Mr. B.'s fancy has led him to imagine, the latter set clearly prove, as I shall endeavor to show in what follows:

The rules of philosophizing seem to have been wholly lost sight of; for, instead of the inference the sanguine Mr. B. draws from 7 and 8, viz., that it can convey double the load, *with half its power*, they prove the fallacy of the preceding—at least, of the inference Mr. B. draws from them; and show a remarkable coincidence with the laws above laid down. In short, these experiments, coinciding as they do with the known laws of falling bodies, ought to have suggested that, there not being a coincidence among all, there existed some defect, mismanagement, or other *adequate cause*, for the apparent disagreement of the first set with known laws: instead, these laws we find virtually pronounced incorrect; because, certainly not that his experiments actually prove them so—for any man reasoning *a priori*, from these laws, would have arrived at results similar to those which the experiments develope—but because, forsooth, it would seem they are *strange* (to him), and perhaps because they would not otherwise account for the preconceived principle of the undulating railway!

The poet may dwell upon the beauties of the visible world, and exclaim with rapture peculiarly his own,

"'Tis distance lends enchantment to the view."

Not so the philosopher. At the distance of 3000 miles he may throw his intellectual ken

across the Atlantic, and pierce the dark atmosphere of an inventor's prejudices in the pursuit of truth. Should the *inferences* drawn from these experiments (the facts are not contested) be true, there would indeed be a revolution, not only in railroads, but in the planetary roads; and possibly render it necessary to remodel those laws which have so long and so well answered the purpose of guiding the planets in their courses!—laws which, we are fain to believe, most of the world would be unwilling to dispense with, even for the bonus Mr. B.'s undulating railway system can confer upon them.

We think that the first of these philosopher's *stones*, of which Mr. B. claims to be the happy discoverer, will be reduced to its original *brass*, by the consideration that the train was projected up the plane by the *momentum* generated by *two* engines, and was assisted in its further progress by one. The *power* then is not as Mr. B.'s fancy has led him to imagine,—"that of one engine doing the work of two on a level,"—but of *one engine aided by the momentum generated by two engines before reaching the starting point*—a power as efficient and active, while it lasts, as that of the engine actually hitched to the train—a power which a form of words cannot annihilate. Had the train advanced to the starting point aided solely by the engine which was to accompany up the plane, we ask, what would have been the result? Why, plainly, the same as in the last set: equal times—equal velocities.

Unless then we can suppose that Mr. B. has discovered some method of condensing large quantities of momentum, which he may carry in his pocket, and infuse into his train before encountering one of the "bumps" of his "undulating railway system," our humble opinion is, that he will have to amend his first inference by striking out "double the load," and inserting "two-thirds, or one-half of the load," which it is capable of conveying at the same velocity on a level line: which opinion we expect to turn into an indisputable truth in the sequel of this paper.

Again, searching for the cause of the wonder which words have had the power to confer on these experiments—for the reason why, in falling down the plane, the train passes the foot at a *far greater velocity* than at the same point when ascending,—it should be borne in mind that, if the train had proceeded up the plane aided solely by the momentum generated before reaching it, the mass would have advanced until its momentum was exhausted, when, returning, it would have passed the foot of the plane in a time equal to that of its ascent, and at the same speed; adventitious circumstances, such as irregularities of motion, and consequently of friction upon the rails and otherwheres, as well as from the friction of the pistons of the engine, not being taken into account. The rationale of this discrepancy is readily given.

In addition to the momentum generated by the *two* engines, the train was assisted by one up the plane: this would operate to carry the train much farther up the plane than if left to its momentum alone; consequently, even if at the turning point this engine had been detached, the gravitating tendency would have caused the train to repass the foot at a far greater speed than it ascended with—since there was a much longer space in which to generate its velocity by the *accelerating* force. Partly from the same cause, and partly for the reason, that, after the momentum ceased to operate in propelling the load, it must have passed over the remaining distance to the turning point at a very slow rate, the time elapsing in the descent must have been much less than that expended in the ascent—offering, surely, nothing strange or unlooked for, one would suppose, by the experimentor; at least, I can vouch for it, by one who was not *alchemizing* for the "principle of the undulating railway." The experiment was actually more favorable to produce this disparity, *since the engine worked down the plane*, which would tend still farther to increase the difference of the times.

I am therefore quite at a loss to perceive the justice of inferring thence, that *because, under these circumstances*, the train *descends more rapidly*, and in a *less time*, over *a given distance*, "that one engine does the work of two on a level." The only inference warranted by the premises is, *that trains, under like circumstances, will always descend more rapidly and in less time than they will advance up a given inclination.* It is only by a computation of the performance of the engines in the experiment, and a comparison with the actual performance on a level, that we can pronounce upon their relative performance or advantage.

The last experiments, 7 and 8, show clearly a coincidence between the time of ascent and descent, when the train is influenced by momentum only in propelling up the plane, since the times, as noted, do not sensibly vary from equality; aside from *the working of the engine down the plane*, they would have come quite up to equality.

From this experiment he could only rationally infer, that the times of ascent and descent of planes under these circumstances is *nearly a ratio of equality:* and not that, "on the undulating plan, one engine can accomplish double the work *with half its power*," since the *relative* performance can only be ascertained by a comparison of the computed *absolute* performances on the plane and on the level. These computations I shall presently submit.

In short, the inferences Mr. B. draws from the experiments are about as much warranted by the premises as one would be in estimating the value of a quantity of vegetables from the known value of a pound of cheese.

We have seen that the fact of the descent being performed in less time than the ascent in these experiments, under these circumstances, is nothing strange, nothing new, and is consistent with the laws of dynamics; that the last experiment is a direct and plain corroboration of these laws; that the inferences drawn

from both sets are incorrect; that, consequently, any superstructure which may be erected upon them is—un chateau en Espagne.

These facts will appear more striking when I shall have answered the question—"What *practical result* may then be inferred from them?"

Submitting the performance of the first to calculation, we shall find that the rate exceeds but little over 12 *miles per hour;* but if the engines had been permitted to continue their course on the level, it would have been 19 *miles per hour.* We infer, then, that if a train of cars of the weight, and under the circumstances, of those in the experiment, be brought to the foot of a plane similar to the Sutton plane—if then one engine be detached, while the remainder proceeds up the plane until the gravity of the whole exhausts both the momentum generated by the two engines and the power of the accompanying engine—and at this point the power of the engine be reversed, and the whole return down the plane—that there will *always* be a *loss* by the *use of this method*, compared with that of the *two engines working on a level road*, in the ratio of 2 : 3, or one-third.

If we submit the performance of the two last to calculation, we shall find a result *below* 10 *miles per hour* for the actual performance. The performance on the level would have been 18 *miles per hour.* We infer then that there is a *loss* in the *use of the principle of "the undulating railway,"* under the circumstances of the experiments 7 and 8, in the ratio 10 to 18, or one-half.

As to curvature—shade of Newton!—that matter may be moved in a curved track at a *greater velocity* and *with less power* than in a straight line! is to say that bodies moving in curved paths do not always require an increase of power to propel them in the *direct* ratio of the *squares of the velocities*, and in the *inverse* ratio of the *radius of curvature!*—is to say that the more the curvature is sharpened, the greater the facility with which a body moves therein! (what a rapid flight we might make to the moon by a road curving like a succession of S's!)—is to say, that when a single force is resolved into its *components*, that a *part* is greater than the *whole!!*

Did the English Smeatons and Rennies, and the French Pronys, seriously approve of all this? or did they smile and shake their heads?

Albany, December 19, 1833.

Further Experiments on the Liverpool and Manchester Railway, to determine the correctness of the Undulating Railway System.
[From the London Mechanics' Magazine.]

Sir,—I have this morning (29th October) received your numbers of the 19th and 26th inst. My principal object in now addressing you is to make an observation or two, which Mr. Cheverton's last communication immediately demands. In the course of a few days I shall trouble you with more general remarks in answer to "S. Y.'s" objections, to which the contents of your next number may possibly enable me to add something in further reply to Mr. Cheverton.

Mr. Cheverton says, in allusion to the comparative amount of resistance on the two lines, "Mr. Badnall will not undertake the task (of showing how trivial is the difference), because it will *disclose the poverty of his scheme.*" In the same breath, also, he observes, that "space will not allow him (Mr. Cheverton,) to show how trivial one is in comparison to the other."

Thus Mr. Cheverton, in a most unwarrantable manner, accuses me of withholding the truth when I have the power of publishing it, which *truth, if exposed,* would (he says) prove the poverty of my scheme; and yet, after nearly a *twelvemonth's* discussion, he, *for want of space*, declines to touch upon this subject. I appeal to all your readers, *is this generous?*

But Mr. Cheverton endeavors to sweeten this bitter observation, by saying, "I cannot believe that there is any intention to deceive, *yet it suits his* (Mr. Badnall's) *purpose better, (though it is not dealing fairly with the public,) to take a very short distance*, such as 147 yards," &c. &c.

There is no intention of deceiving, and *yet I am not dealing fairly with the public!!* I trust Mr. Cheverton will have the good feeling to revoke these expressions, or, at least, to explain them; and if not, I trust he will exclude Mr. R. Stephenson, Mr. Dagleish, Mr. Dixon, and the other engineers under whose joint inspection and superintendence most of the experiments were made, from any participation in a wish *to deal unfairly with the public.*

I am not one, Mr. Editor, who feels disposed to quibble about trifles, or, in discussions of this kind, to be disturbed by every burst of anger from an opponent whom, in this instance, *I feel within my grasp;* but I offer my unqualified protest against the right or propriety of any man attributing *unjust motives* to another, without a cause which he is able to substantiate.

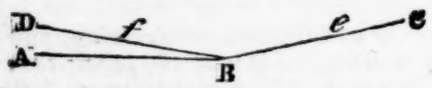
Mr. Cheverton's remarks about inertia and gravity are becoming as familiar to me as my "*hic, hæc, hoc.*"

His simile of "the two boys with a swing," would, I should have imagined, (as some time ago alluded to by your correspondent "Saxula,") have led him to reflect rather more deeply on the subject before us. Had I told him, before this discussion was entered upon, that I could, by means of my own arm, and without the aid of any assistant artificial power, raise a ton weight above the level of my head, he would not, perhaps, have believed me; yet he knows very well, that if a ton weight were suspended but a few inches from the earth by a rope of sufficient strength and length, and from a prop of sufficient strength, there could be no difficulty, by gradually increasing the oscillations of the load, in attaining the required elevation,—nay, by the simple application of an equally simple contrivance, to retain the load at that very elevation when attained. Now, although throughout each oscillation of this pendulum, the effect of gravity from each de-

scent of the weight may be said to be destroyed in the corresponding ascent, would the power of gravity, or would it not, be auxiliatory in the accomplishment of this task?

With regard to 147 *yards* being too limited a distance to suit the trial of a fair experiment, Mr. Cheverton must blame the locomotive engine, not me. The inclined plane is 1¼ mile in length, and if the Rocket engine had not sufficient power to reach a higher elevation, was it *my fault*? The experiments which were *last* tried will surely satisfy Mr. Cheverton that he has done me injustice, in supposing that I *chose* the *worst engine*, from any supposition that *it would best support my arguments on this subject.*

As it appears that the deductions which are drawn by engineers from these experiments, are not fully understood by many who have perused the particulars of them, I shall beg your insertion of a diagram, published by the editor of the Manchester Guardian, which will probably be found to render my ideas as clear as they can be rendered on this subject:



A B is a level line—B C, equal to B D, forming an undulation D B C, whose summits are of equal altitude.

The train of loaded carriages, weighing 150 tons, and moved by two engines, one dragging, the other propelling the load, had acquired from A to B (the length of this line being one mile on the railway,) a velocity of 20.28 miles an hour. On arriving at B the steam of the propelling engine was shut off, and the engine stopped. The train, then, partly by the momentum acquired in travelling over the level railway, and partly by the power of the remaining engine, ascended the inclined plane to the point C, viz. 575 yards, which was as far as the united locomotive force of one engine and the momentum could carry it. The power was then reversed, and the engine pushed the carriages back from C down the inclined plane to B, at which point it was found that the descending velocity was 31.70 miles an hour. Now, as the velocity of 20.28 miles an hour at B was sufficient, with the aid of *one engine*, to carry the whole train from B to C, what man living will dispute that a velocity of 31.70 miles an hour, at the same point, would, with the same engine, have enabled the train to ascend the line B D, which is supposed to be precisely equal to the line B C? On the contrary, would there not be *a given velocity* generated at the point D, which, the effective power of the engine being still continued, would have enabled the train to pass over another *like* undulation? If so, one engine could move 150 tons along the undulation C B D, which amount of tonnage she could not move on a level road, and which *on a level road, whatever velocity was given to the train at starting, would bring the engine gradually to a halt.*

But let Mr. Cheverton direct his attention to the two last experiments which were tried. If *a momentum* (*momentum only*), arising from a velocity at the point B of 19.04 miles per hour, carried the train 323 yards—say as far as *e* on the inclined plane B C—would not a momentum, arising from a velocity of 20.04 miles per hour, carry the same train to the point *f* up the ascent B D? If so, the Firefly engine proved her capability of moving 150 tons along an undulation *e* B *f*, by the employment of her power throughout only *half* the distance, and if this were the case over *one* undulation, will Mr. Cheverton deny that *such would have been the case over succeeding undulations*?

During the next week I may, perhaps, have an opportunity of trying a few more experiments: if so, they shall be transmitted to you. I have expressed my opinions to the *Liverpool Directors*, that the *Firefly engine* is capable of conveying from one summit of an undulation to another the enormous load of from 200 to 250 tons. I may have exceeded the limits of her power in this prophecy; but will Mr. Cheverton admit one point before he hears the result of any further experiments? viz. that *if the Firefly*, or any other engine, can move even 200 tons from summit to summit of *one* undulation, that she is capable of moving an equal load from one summit to another summit of a *second* or greater number of like undulations?

If so, what becomes of his weakest and most untenable argument, that *all I gain* is the comparatively *trifling advantage which gravity gives me at the commencement of locomotion?* I am, Sir, with great respect, your very obedient servant, RICHARD BADNALL.

October 29, 1833.

N. B.—The subject of friction, (trivial as Mr. Cheverton thinks it,) I shall not be ashamed to discuss in my general answer to "S. Y."

The Late Experiments made on the Liverpool and Manchester Railway, to determine the accuracy of the Undulating Railway Theory.

[From the London Mechanics' Magazine.]

SIR,—The truth of Mr. Badnall's statements of the experiments on the Sutton inclined plane being of importance to those who have no other means of forming their judgment upon the undulating railway theory, I take the liberty of asking that gentleman, through the medium of your Journal, which contains his account of the trials on the 23d and 24th of September (see No. 530,) how it happens that your printed statement doth vary from the verbal one which he was so polite as to give me at the Star hotel, Liverpool, on the evening of the said 24th, in the presence of Mr. Perkins the engineer, and several other gentlemen? Mr. Badnall read from his note-book, carefully and distinctly, while I wrote down his words (in ink), which, at my request, were repeated by him to prevent mistakes, and I have now the memorandum before me. Mr. Badnall will recollect, that I told him *the same night* that the results did not accord with each other, if the inclination of that part of the Sutton plane,

upon which the experiments were tried, was really as great as he had assumed it to be—namely, 1 in 96.

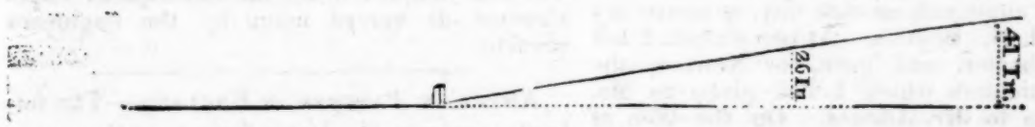
The statement which Mr. Badnall then deliberately gave me was as follows, and refers only to the first, second, and eleventh experiments:

"Weight of the Rocket engine and tender, 6½ tons; wheels 4 feet 8 inches *diameter*. *First*—Began the ascent (without steam) at the rate of 18 miles an hour (equal to 18 *strokes of the piston in* 10 *seconds*), and ran up the inclined plane to a distance of 217 yards, by *momentum*. *Second*—Returned to the foot of the inclined plane (using the full power of the engine), and acquired a velocity of 22 miles an hour (or 22 strokes of the piston in 10 seconds); shut off the steam, and ran, on the level plane, to a distance of 454 yards, by *momentum*. *Eleventh*—Two engines took a load of 35 tons on the level, and reached the foot of the inclined plane with a velocity of 12 miles an hour (or 12 strokes of the piston in 10 seconds). One of the engines being left behind, the other (the Rocket) shut off the steam, and ascended a space of 177 yards, by *momentum*."

The statement with regard to the *last* experiment, it will be seen, agrees with your printed account; but the two first do not, as to the fact of the acquired velocity; yet it is a truth which Mr. Badnall will not deny, that I at first understood him to say "*twenty to twenty-two miles an hour;* but, upon my repeating those words, thinking the expression too vague, he corrected me or himself by saying, "*twenty-two precisely.*" I feel sorry for the necessity of calling upon Mr. Badnall to explain how it was that, four days afterwards, he should send you so different an account for publication. Did he mean to deceive me, or your readers? In either case, I opine, he will be considered as having rather too much of "the man of the world" in his composition. I should have taken this step before now, had I seen the paper in No. 530 sooner; but I did not get it till the 2d inst. in the last monthly part, and have since then been closely engaged in my regular vocation.

Being desirous of finding out the error, for I felt assured of its existence, and Mr. Badnall having returned to the Isle of Man, I wrote to Mr. Booth on the subject, of which letter the following is a copy:

"Sir,—Having been invited by Mr. Badnall to witness the experiments on the Sutton plane on Tuesday, the 24th ult., I went there on that day, but, as you will probably recollect, was too late for personal observation; for, at the moment I arrived, the Rocket engine had received some damage, and Mr. Badnall, with yourself, was on the point of returning to Liverpool by the same train. Mr. Badnall gave me the principal results on our arrival at his hotel in Liverpool, which I cannot at all reconcile, upon the supposition that the inclination of *that part* of the Sutton plane is 1 in 96. Perhaps the quarter-mile post which you assumed as the *foot* of the plane is not precisely so, or some alteration may have taken place since their first construction. If accurate levels are taken, I am inclined to think the section will be found as follows:



"Should you think it necessary to have actual levels taken for the purpose of corroborating the other facts, will you have the goodness to favor me with the result? And you will further oblige by informing me when you next try experiments with reference to the undulating theory, and by allowing me to be present during the trial. I am, sir, your obedient servant, H. S.

"Sheffield, Oct. 1, 1833.

"*Postscript.*—I ventured to tell Mr. Badnall on the same evening, that there must be some material error, and that I thought it was in the assumption of 1 in 96 for the inclination. This well-meant observation, however, did not appear very palatable.

"To Henry Booth, Esq., Treasurer to the Liverpool and Manchester Railway Company."

It is to be regretted, for the sake of truth, and Mr. Booth's acknowledged character as a gentleman and a lover of science, that all the notice of this communication which I have had the honor to receive, is contained in a postscript of an unlooked-for letter from Mr. Badnall, dated October 19th. He says,—"Your letter to Mr. Booth may lead you to a better acquaintance with the precise inclination of the Sutton inclined plane; but, however the inclination may vary from the reputed rise, it cannot affect the proportionate result of experiments." True, it cannot; but it affords the means of *checking* the statement of Mr. Badnall; and there would have been another check, if that had been done which on the 24th September I said ought to have been done, namely, accurate observations recorded of the *length of time* occupied in traversing the respective spaces by means of the acquired *momenta*. This hint, it appears, has not been forgotten in trying the subsequent experiments, and its being acted upon may be attributed to Mr. Robert Stephenson, of Pendleton, who perfectly agreed with me in opinion as to its propriety. [See Liverpool Mercury of October 18.]

As soon as I am more at leisure I shall send you some further ideas on the undulating railway theory, which I will endeavor to condense as much as possible, and will not forget the friendly notices of your correspondents "Junius Redivivus" and "S. Y." Yours, respectfully,

Henry Sanderson.

Sheffield, Nov. 10, 1833.

Mr. Badnall's Explanation of the Alleged Discrepancies in the Reports of the Recent Experiments on the Liverpool and Manchester Railway, to determine the correctness of the Undulating Railway Theory.
[From the London Mechanics' Magazine.]

Sir,—I feel indebted by the opportunity you have afforded me of explaining the cause of the difference between the statements alluded to by Mr. Sanderson, and those sent by me to you for publication. I have no doubt that Mr. Sanderson copied my observations correctly; and those observations, at the time I communicated them to him, I believed to be perfectly correct. Mr. Sanderson is aware that, on the day the experiments were tried, I was laboring under severe indisposition; and, as the weather was extremely inclement, I did not take the same active part in the experiments as Mr. Booth, Mr. Rae, and Mr. Scott. The observations I made, as they appear in my note-book, in regard to the velocity of the engine and train at the foot of the ascent, were founded on my own calculation of the number of strokes which the engine appeared to be working when she passed the spot on which I stood. The spaces passed over were measured, and the particulars agree with the statement which I gave to Mr. Sanderson. Immediately after the experiments were concluded I proceeded to Liverpool with Mr. Sanderson, whom I met in one of the railway carriages; and the same evening I gave him the particulars to which he alludes. He will perfectly recollect that the following day I was not, through indisposition, able to get up till one o'clock; and, having an arrangement at Manchester, I could not, on that day, compare my notes with Mr. Booth's. At *two* o'clock I left for Manchester, and gave, at Newton, the same particulars which I had given to Mr. Sanderson to *Mr. Allcard.* On the 26th of September I left Mr. Sanderson in Manchester, and returned to Liverpool; and, previously to addressing you, I considered it better to have an interview with Mr. Booth, who was in possession of the notes taken by Mr. Rae and Mr. Scott, and who had himself carefully taken down all the particulars. At this interview I found that I had over-estimated the velocity at which the piston was moving; and the statements, therefore, which I sent to you were *not my own*, as given to Mr. Sanderson, but those of three other individuals, which decidedly told more *against my principle* than *my own* would have done.

For instance, had the engine been travelling at eighteen miles an hour at the foot of ascent, before rising the hill, and twenty-two miles an hour at the same point, after descending the hill, which are the particulars given to Mr. Sanderson, the result would have shown a greater gain by the undulating system than when the velocities were fifteen strokes of the piston (or about fifteen miles an hour) before ascending, and sixteen strokes of the piston about sixteen miles an hour) after descending, s published in your Magazine.

I have only to add, that in all the statements of the experiments which I *have sent to you*, I have cautiously avoided laying myself open to the slightest charge of error or partiality. On the contrary, my notes have always been corrected by, and compared with, the notes of others.

With regard to the particular inclination of the Sutton inclined plane, it has always been understood to be about *one* in ninety-six. After my first experiments were made, however, the levels were taken afresh, and it was found, that towards the foot of the plane the inclination was considerably less than the average rise. For instance, from the point from which the ascent of the Rocket engine, &c. was calculated, the plane rises as follows:

1st	88 yards,	- - - - -	1 in 122.
2d	do.	- - - - -	1 in 105.
3d	do.	- - - - -	1 in 97.
4th	do.	- - - - -	1 in 94.
5th	do.	- - - - -	1 in 92.
6th	do.	- - - - -	1 in 89.
7th	do.	- - - - -	1 in 89.

And the entire distance here denoted exceeds that to which, in any of the experiments, the train ascended.

It must, however, be evident, that however a variation in the inclinations may affect Mr. Sanderson's calculations, it cannot possibly,—which he will, no doubt, allow,—affect the comparative results of the experiments.

I remain, sir, yours, &c.,
Richard Badnall.

Farm-hill, near Douglas, Nov. 19, 1833.

N. B.—There can be no doubt as to the measurement of time being the best test; which test was adopted in all the subsequent experiments, as agreed upon by the engineers present.

American Patents in England.—The following extract of a letter from a gentleman in England, to his friend in this country, may be useful to those who desire to take out patents in Europe.

Extract of a letter from London, dated 14th December, 1833.

"It may be of use to apprize inventors of mechanical improvements in the United States, that it is essential to the security of their interests, if they design to take out patents for their inventions in this country, not to disclose the secret of their inventions in the United States until they have secured a patent here. There are many ingenious mechanics in the United States, in correspondence with their friends in this country, constantly upon the watch to seize any thing new and likely to be useful, and to transmit the particulars to their friends, and thus forestal the rights and interest of the original inventor."

New Ink Distributor.—We were lately shown, by Warren Jenkins, of this town, a new or improved machine, invented by himself, for regulating, with mathematical exactness, the distribution and application of ink to

the types. This machine is the first attempt of the kind by the inventor, and, therefore, imperfect in some of its mechanical details; but, in its present condition, it answers the intended purpose very well. We have scarcely a doubt that the principle upon which the machine is constructed is correct, and that a more perfect mechanical construction of all its parts will make it fully to answer the purposes for which it is designed.

Should our anticipations be realized, and there seems little room to doubt that they will, an ordinary pressman, with the assistance of a boy capable of moving the *rollers*, can take any number of *clear*, *uniform* impressions, extending throughout a whole *form* or a whole book. This machine will, therefore, be no less important to readers who desire beauty and uniformity in what they read, than to printers who are responsible for the quality of their work.

When the machine is once regulated so as to take a suitable quantity of ink, which will be the labor of but a few minutes, the pressman is relieved from further care in relation to the uniformity of the impressions, as the application of the ink to the rollers is entirely beyond the control of the boy who does the rolling. We may also add, that the machine is of simple construction, and of course, little liable to get out of order; whilst the cost will probably place it within the reach of every person owning a press. Several other advantages might be enumerated, but we think enough has been said to entitle it to the consideration of printers.—[Cincinnati Journal of Science.]

Straw Cutting Machine.—A few weeks ago, we purchased for a subscriber on the eastern shore of Maryland, one of Sinclair & Moore's straw cutters, of the middle size. We this week received from him the following testimony of its excellence, which we publish rather for the benefit of the public, than for that of the worthy manufacturers. By the way, it seems to be universally allowed by all who have tried it, that the saving by the cutting of *all long food* for stock is a very important one. We have noticed the testimony of farmers, of graziers, of dairy-men, and of stage proprietors, all to the same point, and have never heard one state a contrary opinion against it. Those who have tried steaming this cut food, seem to agree pretty unanimously that this operation improves its quality still as much more. Surely these facts are worthy the careful attention of all concerned in the feeding of cattle and horses—but here is an extract from our friend's letter:

"I am delighted with my straw cutter. It will have saved all the money it cost before May-day. The 'meat, drink, washing and lodging,' of *each* of my carriage horses, cost me the last year at least $100: by means of the straw-cutter, I can give them better board, keep them sleeker and happier; and (at the present price of produce) *both* of them, the current year, will not cost me more than $60. I shall be the means of procuring a market for several straw-cutters, at which I rejoice, not on account of the seller, but the buyer.—[Amer. Farmer.]

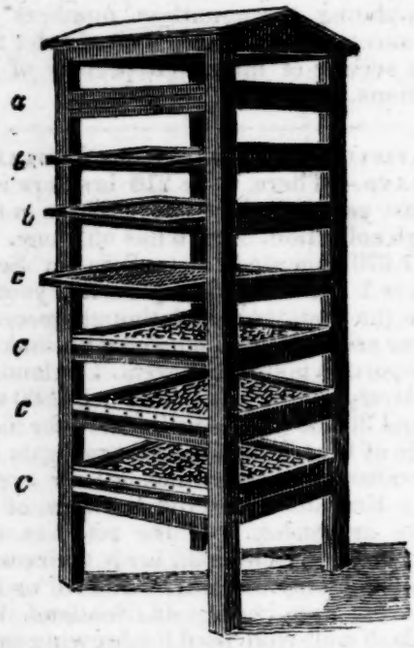
Consumption of Silk.—The quantity of this material used in England, alone amounts each year to more than four millions of pounds weight, for the production of which, myriads upon myriads of insects are required. Fourteen thousand millions of animated creatures annually live and die to supply this corner of the world with this luxury! If astonishment be ascribed at this act, let us extend our view to China, and survey the dense population of its widely spread region, whose inhabitants, from the emperor on the throne to the peasant in the lowly hut, are indebted for their clothing to the labors of the silk worm. The imagination, fatigued with the flight, is lost and bewildered in contemplating the countless numbers which every successive year spin their slender threads for the service of man.—[Repertory of Patent Inventions.]

Statistics of Brewing in England and Scotland.—There were 216 brewers in Scotland last year, of which above 33 are in the Edinburgh collection. Argyll has only one. There are 17,070 licensed victuallers in Scotland, which is 1 for every 123 persons, young and old, in the country; and though grocers who sell beer are evidently included with innkeepers, the proportion is still very great. England, which is a *thirsty* country, rejoices in 50,800 victuallers, and 30,000 "persons licensed for the general sale of beer," making an aggregate of 81,700 retailers of beer, which is 1 for every 170 souls. England has 1753 brewers, of whom 108 are in London. Of the retailers of beer, 37,000, or nearly one-half, brew their own beer. In Scotland, only 318 out of 17,070, or 1 in 57, brew their own beer. In Scotland, 990,000 bushels of malt were used for brewing in all the 16 collections, of which one-tenth was used by the licensed victuallers; 432,000 bushels were used in the Edinburgh collection; 62 bushels served the two collections of North and South Argyll, containing 100,000 souls. In England, 25,800,000 bushels of malt were consumed in the manufacture of beer: 13,800,000 by the brewers, and 12,000,000 by the victuallers or other retailers. In Scotland, the malt brewed is at the rate of four-tenths of a bushel for each person; in England it is 1¾ bushel. Ireland consumed 1,540,000 bushels of malt in her breweries, which is about two-tenths of a bushel for each person. Of brewed liquor, *one* Englishman drinks as much as *four* Scotchmen, or *nine* Irishmen. In 1831, there were 928,000 bushels of malt used for brewing in Scotland, of which 834,000 were used by the brewers, the rest by victuallers. In 1830, the Scotch brewers consumed 740,000, but the paper from which this is taken does not mention the victuallers. The increase in the quantity of malt used by the brewers since 1830 seems to be about *one-fifth*.

Rearing of the Silk Worm. [From the New-York Farmer.]

Apartments.—Almost any building will answer for rearing the silk worm; a barn, garden, apartments of the dwelling-house, provided they are dry, can be properly ventilated without exposing the worms to chilling currents of air, and kept free from insects, mice and other vermin. Boards, shelves, tables, or shallow boxes, will answer in absence of other more convenient apparatus.

The apparatus of the Rev. Mr. Swayne is recommended from its simplicity and convenience—rendering the apartments capable of being easily kept clean. It is a wooden frame, four feet two inches high, each side sixteen and a half inches wide, divided into eight partitions, by small pieces of wood, serving to form grooves, in which the slides, containing the worms, run in and out. The upper slide, *a*,



is of paper, designed to receive the worm as soon as hatched. The two next, *b*, are of cat-gut, the threads about one-tenth of an inch apart. These are for the worms a little more advanced. The four lower ones, *c*, are of wicker work, split rattan, or netting, with openings of a quarter to half an inch square. Under each of these is a slide of paper, to prevent the dung from falling on those below. Spare drawers should be made for the two upper ones. When the worms are grown so large as to require more room, they are removed to large drawers of netting, or wicker work, where they remain until they show signs of a desire to spin.

The Eggs.—The fecundated eggs turn from a pale yellow color to a brownish slate color. They are kept in a convenient place—in a dry cellar, in a box, where they will not receive injury from mouldiness, and from mice, until the mulberry begins to throw out leaves. They are then placed in the kitchen, or other suitable place, where the warmth of the air will hatch them in a day or two.

Feeding the Worms.—It is desirable to have all the worms designed to be reared, hatched at the same time. As soon as they appear, the tender leaves, or trimmings of the mulberry, are gently laid over them. If they are fed with leaves they will require a fresh supply three times a day; if with the trimmings, once or twice will be sufficient. To prevent the leaves from becoming dry, they are kept in a cool place, in a glazed vessel, or by laying a board over them. The leaves, if gathered in wet weather, should be dried before they are given to worms. It is best, therefore, to gather a sufficiency for three or four days at once in fair weather. The older the worm the larger the leaves suitable for them.

Cleaning.—The shelves, or whatever the worms are placed on, should be carefully and frequently cleansed. Sometimes the boards are covered with old newspapers, which are turned and renewed. The worms can be changed from one part of the shelves to another by taking them up in the fingers, or by removing the leaves on which they are eating, and thus affording an opportunity to brush away the litter.

Different Ages and Moulting.—The most common varieties, of which the large white is recommended as the best, change their skins four times. These changes, called moultings, divide the life of the worm into five ages. The first moulting generally takes place on the fifth or sixth day after hatching, the second on the eighth, the third on the thirteenth or fourteenth, and the fourth on the twenty-second.

Before the worms moult or change their skins, they are less active, and indicate sickness or distress, by raising their heads. They should, in this situation, be disturbed as little as possible, and fed very sparingly.

Spinning.—When disposed to spin, the worms, true to the laws of their nature, will cease to eat, and begin to wander about. They now should be immediately supplied with conveniences for spinning, by placing on their shelves regularly made handles, brushwood, or broom corn. The last, if properly secured and spread out or pressed against the upper shelves, as recommended by G. B. Smith, Esq., of Baltimore, are the best. The worms will readily ascend these little trees, and diligently commence their useful work. Each one will spin its cocoon in three or four days; and in about six or eight days, they all will have finished their labor.

Breeding or Seed Cocoons.—The largest, best formed, and most firm cocoons, should be selected for the eggs of the future brood. The coarser floss, or wool silk, may be taken off, and hung on a thread, by the aid of a needle, without piercing the cocoons. In about a week or more the moths or butterflies will eat their way through the cocoon. They will now couple, and, by taking them in pairs, by the wings, they may be put, as many as convenient, on sheets of paper, where they will remain and deposit their eggs. When they are done laying they soon die. The sheets can be folded up,

and put in a dry and cool place until the following spring. If allowed to remain in a warm room they will hatch. The sun should not be allowed to shine upon them at any time ; and the room where the millers lay their eggs should not be molested by cats, mice, or insects.

Curing Cocoons.—When the cocoons cannot be reeled immediately after they are spun by the worm, the encircled chrysalis must be killed to prevent it from eating out, and thus destroying the cocoon for reeling. They are put in shallow vessels, or in baskets, and then placed in an oven moderately heated, to remain half an hour. Another method is steaming them. Mr. Smith, of Baltimore, recommends them, when raised on a small scale, to be put in a tight tin vessel, which is surrounded with another containing water heated, until the cocoons become as hot as boiling water. After this, they are spread out to dry. The per-centage lost in the weight by curing is about 25, depending on the dryness. The cocoons will reel better immediately after they are finished by the worm.

Space for the Worms.—It is desirable that they should not touch each other. An ounce of good eggs will produce about 40,000 worms. The space these will require in their five ages may be in a proportion not far from the following square feet, 8, 16, 36, 86, 186.

Diseases of Silk Worms.—These are described as being numerous, and sometimes very destructive ; but wholesome air, perfect cleanliness, avoiding feeding with wet leaves, are preventives. Chloride of lime has been recommended by some to purify the rooms, and others have found it injurious. If used, it should be sparingly. The fifth age is considered the most critical.

Quantities of Silk from Cocoons.—The estimates, in respect to the silk from the cocoons, are various, depending on their size and quality. We obtained one pound (16 oz.) of sewing and gloss silk from one bushel, containing 1940 cocoons, which were two years old. Mrs. Williams obtained nearly one and a half ounces from 244 cocoons. Miss Rhodes one ounce from the same number. The calculation of those who have had experience is from 2000 to 4000 cocoons for a pound of silk.

Profits.—It is very difficult to make an estimate of the profits from raising silk worms. It is a fact that our climate is congenial to the worm and the mulberry. It is another fact that many millions of money are annually expended for foreign silk in various states of preparation and manufacture. These facts afford ample encouragement. The actual profits will depend on various other circumstances : whether the trees are set in lanes, avenues, unoccupied grounds, hedges, or in orchards—whether carried on a large or small scale—whether expensive or cheap labor is obtained. Like all other pursuits, its success depends on care and attention. The Morus multicaulis will undoubtedly be the most profitable tree to feed the worms.

The following estimate was by John Fitch, Esq. of Mansfield, Ct., in the Manual published by Congress. One acre of full grown white mulberries, set at the distance of one and a half rods, will produce 40 lbs. silk. For the first three weeks after the worms are hatched, one woman will do all the labor. For the next twelve or fourteen days five persons are required. During this last period there should be one or two men. If children are employed, a greater number than five are required. The picking of the balls and reeling the silk can be done by about the same number of women and children in ten to fifteen days. The lowest cash price is $5 per pound.

40 lbs. at $5 - - -		$200
Labor and Board -	$80	114
Spinning - - -	34	
Net profit per acre - -		$88

Female Education.—Let your first care be to give your little girls a good physical education. Let their early years be passed, if possible, in the country, gathering flowers in the fields, and partaking of all the free exercises in which they delight. When they grow older, do not condemn them to sit eight listless hours of the day over their books, their maps, and their music. Be assured that half the number of hours passed in real attention to well ordered studies, will make them more accomplished and more agreeable companions than those commonly are who have been most elaborately finished, in the modern acceptation of the term. The systems by which young ladies are taught to move their limbs according to the rules of art, to come into a room with studied diffidence, and to step into a carriage with measured action and premeditated grace, are only calculated to keep the degrading idea perpetually present that they are preparing for the great market of the world. Real elegance of demeanour springs from the mind ; fashionable schools do but teach its imitation, whilst their rules forbid to be ingenuous. Philosophers never conceived the idea of so perfect a vacuum as is found to exist in the minds of young women supposed to have finished their education in such establishments. If they marry husbands as uninformed as themselves, they fall into habits of insignificance without much pain ; if they marry persons more accomplished, they can retain no hold of their affections. Hence many matrimonial miseries, in the midst of which the wife finds it a consolation to be always complaining of her health and ruined nerves. In the education of young women we would say, let them be secured from all the trappings and manacles of such a system ; let them partake of every active exercise not absolutely unfeminine, and trust to their being able to get into or out of a carriage with a light and graceful step, which no drilling can accomplish. Let them rise early and retire early to rest, and trust that their beauty will not need to be coined into artificial smiles, in order to secure a welcome, whatever room they enter. Let them ride, walk, run, dance, in the open air. Encourage the merry and innocent diversions in which the young delight ; let them, under pro-

per guidance, explore every hill and valley; let them plant and cultivate the garden, and make hay when the summer sun shines, and surmount all dread of a shower of rain or the boisterous wind; and, above all, let them take no medicine except when the doctor orders it. The demons of hysteria and melancholy might hover over a group of young ladies so brought up, but they would not find one of them upon whom they could exercise any power.—[For. Quar. Review.]

ACCOUNT OF A LIBRARY FOR WORKING MEN.—A correspondent, who gives his name and address, has been induced, by our notice of Sir J. Herschell's Address to the subscribers of the Windsor and Eton Public Library, (see p. 245, vol. ii. of this Magazine,) to send us an account of a similar institution on the Borders, with which he had been himself connected. He states that a gentleman, well known for his enlarged views of the state and prospects of society, being one evening in the place, was led to inquire whether there was any public library in the town. He was informed in reply, that there was one of ample extent, the entrance-money to which was £5, and the annual payment 14s. Feeling this to be beyond the reach of the poor, he inquired if there was no other library. He was told that there was the "Tradesman's Library," the entrance to which was £1, and the yearly payment 4s. This was nearer the point certainly, but still did not exactly meet the views from which this gentleman's inquiries had proceeded. "It will not supply the young," was his reply; "you must try another, to excite the desire of knowledge among the young and the poor." The minister of the parish, his lady, and a few other persons, adopted the suggestion. In a few days £20 were freely and readily given, and the donors were called to a meeting in the town-hall. At this meeting some were for allowing to the readers the gratuitous use of the books, but the majority very properly doubted the prudence of this plan, and it was decided to cherish the natural desire of independence in the poorest and youngest by requiring the payment of a penny monthly. It was also agreed that the volumes should be of small extent, that they might be returned once a month or oftener. The sum raised procured about eighty volumes, and a donation from the first mover of the plan added twenty or thirty more. The second week after the commencement there were above one hundred applicants, of whom about thirty were poor laborers or solitary females, and a larger number were under fourteen years of age. Numbers of them had not read two hours in succession for many years before. At the beginning of the second year the readers were allowed, at their own desire, to pay for six months at once, instead of a penny monthly. Our correspondent relates the following anecdote, which illustrates the useful effect of such institutions upon those for whose benefit they are intended.

In the following spring, when the days were lengthened, one of the readers, an agricultural laborer, came with the book he had been using, and declined to take another. He stated that, laboring at a distance for so many hours, he should not be able, during the summer, to indulge his desire for more reading. On being asked if he thought his monthly penny had been well spent, his hard countenance assumed the air of one who had found a treasure as he replied—"Had I paid you a shilling a week instead of a monthly penny, myself and family would have been gainers. During the winter months I and those like me got home and took dinner between four and five o'clock. Then an ill-ordered house and noisy family induced me and others to go out. If the weather was favorable, we stood to talk and spend an hour at the Cross; if otherwise, we went into a smithy for shelter, and often to the public-house, and, though I am not given to drink, yet we had to spend a little when there, and even a little frequently occurring is felt by a poor man. When I took home my first book from the library I was asked to read aloud, but objected because of the noisy children. After some time, the younger were put to sleep, and I began to read. Next morning, and every evening after, my house was clean and in order, the fireside trimmed, my meal waiting, the children in bed, or allowed to sit up on condition of listening as quietly as their attentive mother. The book we obtained from the library was Goldsmith's 'Animated Nature,' and it has been highly interesting to us. And, Sir, apart from all we have learned by reading, to find, week after week, my own house the most comfortable, and my own family the happiest, I ever saw, shows me that a poor man with his book in his hand may be as happy as the richest or most noble." This man concluded with assuring our correspondent that he had heard from others statements similar to that which he had made for himself.

Animal Mechanics, or Proofs of Design in the Animal Frame. [From the Library of Useful Knowledge.]

(Continued from page 115.)

Consider two men wrestling together, and then think how various the property of resistances must be: here they are pulling, and the bones are like ropes; or again, they are writhing and twisting, and the bones bear a force like the axle-tree between two wheels; or they are like a pillar under a great weight; or they are acting as a lever.

To withstand these different shocks, a bone consists of three parts, the *earth* of bone (sub-phosphate of lime); *fibres* to give it toughness; and *cartilage* to give it elasticity. These ingredients are not uniformly mixed up in all bones; but some bones are hard, from the prevalence of the earth of bone; some more fibrous, to resist a pull upon them; and some more elastic, to resist the shocks in walking, leaping, &c. But to return to the forms. Whilst the centre of the long bones is, as we have stated, cylin-

drical, their extremities are expanded, and assume various shapes. The expansion of the head of the bone is to give a greater, and consequently a more secure surface for the joint, and its form regulates the direction in which the joint is to move. A jockey, putting his hand on the knee of a colt, and finding it broad and flat, augurs the perfection of the full-grown horse. To admit of this enlargement and difference of form, a change in the internal structure of the bone is necessary, and the hollow of the tube is filled up with *cancelli*, or lattice work. These *cancelli* of the bone are minute and delicate-like wires, which form lattice-work, extending in all directions through the interior of the bone, and which, were it elastic, would be like a sponge. This more uniform texture of the bone permits the outer shell to be very thin, so that whilst the centre of the long bones are cylinders, their extremities are of a uniform cancellated structure. But it is pertinent to our purpose to notice, that this minute lattice work, or the cancelli which constitute the interior structure of bone, have still reference to the forces acting on the bone; if any one doubts this, let him make a section of the upper and lower end of the thigh bone, and let him inquire what is the meaning of the difference in the *lie* of these minute bony fibres, in the two extremities? He will find that the head of the thigh bone stands obliquely off from the shaft, and that the whole weight bears on what is termed the *inner trochanter;* and to that point, as to a buttress, all these delicate fibres converge, or point from the head and neck of the bone, which may be rudely represented in this way.

Fig. 10.

The preceding figure exhibits the head of the thigh bone, and shows the direction of the *cancelli*, converging to the line of gravity.

We may here notice an opinion that has been entertained in regard to the size of animals. It is believed that the material of bone is not capable of supporting a creature larger than the elephant, or the *mastodon*, which is the name of an extinct animal of great size, the osseous remains of which are still found. This opinion is countenanced by observing that their bones are very clumsy, that their spines are of great thickness, and that their hollow cylinders are almost filled up with bone.

It may be illustrated in this manner: A soft stone projecting from a wall may make a stile strong enough to bear a person's weight; but if it were necessary to double its length, the thickness must be more than doubled, or a free-stone substituted; and were it necessary to make this free-stone project twice as far from the wall, even if doubled in thickness, it would not be strong enough to bear a proportioned increase of weight: granite must be placed in its stead; and even the granite would not be capable of sustaining four times the weight which the soft stone bore in the first instance. In the same way the stones which form an arch of a large span must be of the hardest granite, or their own weight would crush them. The same principle is applicable to the bones of animals. The material of bone is too soft to admit an indefinite increase of weight; and it is another illustration of what was before stated, that there is a relation established through all nature, and that the very animals which move upon the surface of the earth are proportioned *to its magnitude*, and the gravitation to its centre. Archdeacon Paley has with great propriety taken the instance of the form of the ends of bones, as proving design in the mechanism of a joint. But there is something so highly interesting in the conformation of the whole skeleton of an animal, and the adaptation of any one part to all the other parts, that we must not let our readers remain ignorant of the facts, or of the important conclusions drawn from them.

What we have to state has been the result of the studies of many naturalists; but although they have labored, as it were, in their own department of comparative anatomy, they have failed to seize upon it with the privilege of genius, and to handle it in the masterly manner of Cuvier.

Suppose a man ignorant of anatomy to pick up a bone in an unexplored country, he learns nothing excepting that some animals have lived and died there; but the anatomist can, by that single bone, estimate not merely the size of the animal, as well as if he saw the print of its foot, but the form and joints of the skeleton, the structure of its jaws and teeth, the nature of its food, and its internal economy. This, to one ignorant of the subject, must appear wonderful, but it is after this manner that the anatomist proceeds. Let us suppose that he has taken

up that portion of bone in the limb of the quadruped which corresponds to the human wrist; and that he finds that the form of the bone does not admit of free motion in various directions, like the paw of the carnivorous creature. It is obvious by the structure of the part, that the limb must have been merely for supporting the animal, and for progression, and not for seizing prey. This leads him to the fact that there were no bones resembling those of the hand and fingers, or those of the claws of the tiger; for the motions which that conformation of bones permits in the paw would be useless without the rotation of the wrist—he concludes that these bones were formed in one mass, like the cannon bone, pastern-bone, and coffin-bones of the horse's foot.*

The motion limited to flexion and extension of the foot of a hoofed animal implies the absence of a collar bone and a restrained motion in the shoulder joint; and thus the naturalist, from the specimen in his hand, has got a perfect notion of all the bones of the anterior extremity! The motions of the extremities imply a condition of the spine which unites them. Each bone of the spine will have that form which permits the bounding of the stag, or the galloping of the horse, but it will not have that form of joining which admits the turning or writhing of the spine, as in the leopard or the tiger.

And now he comes to the head: the teeth of a carnivorous animal, he says, would be useless to rend prey unless there were claws to hold it, and a mobility of the extremities like the hand to grasp it. He considers, therefore, that the teeth must have been for bruising herbs, and the back teeth for grinding. The socketing of these teeth in the jaw gives a peculiar form to these bones, and the muscles which move them are also peculiar; in short, he forms a conception of the shape of the skull. From this point he may set out anew, for by the form of the teeth he ascertains the nature of the stomach, the length of the intestines, and all the peculiarities which mark a vegetable feeder.

Thus the whole parts of the animal system are so connected with one another, that from one single bone or fragment of bone, be it of the jaw, or of the spine, or of the extremity, a really accurate conception of the shape, motions, and habits of the animal may be formed.

* For these are solid bones, where it is difficult to recognize any resemblance to the carpus, metacarpus, and bones of the fingers; and yet comparative anatomy proves that these moveable bones are of the same class with those in the solid hoof of the *bellua* of Linnæus.

It will readily be understood that the same process of reasoning will ascertain, from a small portion of a skeleton, the existence of a carnivorous animal, or of a fowl or of a bat, or of a lizard, or of a fish; and what a conviction is here brought home to us, of the extent of that plan which adapts the members of every creature to its proper office, and yet exhibits a system extending through the whole range of animated beings, whose motions are conducted by the operation of muscles and bones.

After all, this is but a part of the wonders disclosed through the knowledge of a thing so despised as a fragment of bone. It carries us into another science; since the knowledge of the skeleton not only teaches us the classification of creatures now alive, but affords proofs of the former existence of animated beings which are not now to be found on the surface of the earth. We are thus led to an unexpected conclusion from such premises: not merely the existence of an individual animal, or race of animals, but even the changes which the globe itself has undergone in times before all existing records, and before the creation of human beings to inhabit the earth, are opened to our contemplation.

Of Standing.—This may appear to some a very simple inquiry, and yet it is very ignorant to suppose it is so. The subject has been introduced in this fashion: "Observe these men engaged in raising a statue to its pedestal with the contrivances of pullies and levers, and how they have placed it on the pedestal and are soldering it to keep it steady, lest the wind should blow it down. This statue has the fair and perfect proportions of the human body; to all outward appearance it ought to stand."

In the following passage we have the same idea thrown out, in a manner which we are apt to call *French*. Were a man cast on a desert shore, and there to find a beautiful statue of marble, he would naturally exclaim, "Without doubt there have been inhabitants here: I recognize the hand of a famous sculptor: I admire the delicacy with which he has proportioned all the members of the body, to give them beauty, grace, and majesty, to indicate the motion and expression of life." But it may be asked, what would such a man think if his companion were to say, "Not at all; no sculptor made this statue; it is formed, to be sure, in the best taste, and according to the rules of art, but it is formed by chance. Amongst the many fragments of marble there has been one thus formed of itself. The rain and the winds have detached it from the mountain,

and a storm has placed it upright on the pedestal. The pedestal, too, was prepared of itself in this lonely place. True, it is like the Apollo, or the Venus, or the Hercules. You might believe that the figure lived and thought; that it was prepared to move and speak; but it owes nothing to art—blind chance has placed it there."*

The first passage suggests the conviction, that the power of standing proceeds not from any symmetry, as in a pillar, or from gravitation alone. It, in fact, proceeds from an internal provision, by which a man is capable of estimating with great precision the inclination of his body, and correcting the bias by the adjustment of the muscles. In the second passage, it is meant to be shown that the outward proportion of the form bears a relation to the internal structure; that grace and expression are not superficial qualities, and that only the Divine Architect could form such a combination of animated machinery.

We shall consider how the human body is prepared by mechanical contrivances to stand upright, and by what fine sense of the gravitation of the body the muscles are excited to stiffen the otherwise loose joints, and to poise the body on its base.

Of the Foot.—Let us take the arrangement of the bones of the foot, according to the demonstration of the anatomists.

They are divided into the *tarsus*, which is composed of seven bones, reaching from the heel to the middle of the foot. The *metatarsus*, which consists of five long bones laid parallel to each other, and extending from the *tarsus* to the roots of the toes. The bones of the toes are called *phalanges*, from being in the form of a *phalanx*.

There are in all thirty-six bones in the foot; and the first question that naturally arises is, why should there be so many bones? The answer is, In order that there may be so many joints; for the structure of a joint not only permits motion, but bestows elasticity.

A joint, then, consists of the union of two bones, of such a form as to permit the necessary motion, but they are not in contact; each articulating surface is covered with cartilage, to prevent the jar which would result from the contact of the bones. This cartilage is elastic, and the celebrated Dr. Hunter discovered that the elasticity was in consequence of a number of filaments closely compacted, and extending from the surface of the bone, so that each filament is perpendicular to the pressure made upon it. The surface of the articulating cartilage is perfectly smooth, and is lubricated by a fluid called *synovia*, signifying a mucilage, a viscous or thick liquor. This is vulgarly called *joint oil*, but it has no property of oil, although it is better calculated than any oil to lubricate the interior of the joint.

When inflammation comes upon a joint, this fluid is not supplied and the joint is stiff, and the surfaces creak upon one another like a hinge without oil. A delicate membrane extends from bone to bone, confining this lubricating fluid, and forming the boundary of what is termed the cavity of the joint, although, in fact, there is no unoccupied space. External to this capsule* of the joint, there are strong ligaments going from point to point of the bones, and so ordered as to bind them together without preventing their proper motions. From this description of a single joint, we can easily conceive what a spring or elasticity is given to the foot, where thirty-six bones are jointed together.

An elegant author has this very natural remark on the joints: "In considering the joints, there is nothing perhaps which ought to move our gratitude more than the reflection, *how well they wear*. A limb shall swing upon its hinge, or play in its socket, many hundred times in an hour, for sixty years together, without diminution of its agility, which is a long time for any thing to last, for any thing so much worked and exercised as the joints are. This durability I should attribute, in part, to the provision which is made for the preventing of wear and tear: first, by the polish of cartilaginous surfaces; secondly, by the healing lubrication of the mucilage; and in part to that astonishing property of animal constitutions, assimilation, by which, in every portion of the body, let it consist of what it will, substance is restored and waste repaired."—[Paley.]

If the ingenious author's mind had been professionally called to contemplate this subject, he would have found another explanation. There is no resemblance betwixt the provisions against the wear and tear of machinery and those for the preservation of a living part. As the structure of the parts is originally perfected by the action of the vessels, the function or operation of the part is made the stimulus to those vessels. The cuticle on the hands wears away like a glove; but the pressure stimulates the living surface to force successive layers of skin under that which is wearing, or, as the anatomists call it, disquamating, by which they mean that the cuticle does not change at once, but comes off in *squamæ*, or scales.

* Demonstration de l'Existence de Dieu, par Fenelon.

* From *capsula*, a little case or box.

The teeth are subject to pressure in chewing or masticating, and they would by this action have been driven deeper in the jaw, and rendered useless, had there not been a provision against this mechanical effect. This provision is a disposition to grow, or rather to shoot out of their sockets; and this disposition to project, balances the pressure which they sustain; and when one tooth is lost, its opposite rises, and is in danger of being lost also, for want of that very opposition.

The most obvious proof of contrivance is the junction of the foot to the bones of the leg at the ankle joint. The two bones of the leg, called the *tibia* and the *fibula*, receive the great articulating bone of the foot (the *astragalus*) betwixt them. And the extremities of these bones of the leg project so as to form the outer and inner ankle. Now, when we step forward, and whilst the foot is raised, it rolls easily upon the ends of these bones, so that the toe may be directed according to the inequalities of the ground we are to tread upon; but when the foot is planted, and the body is carried forward perpendicularly over the foot, the joint of the leg and foot becomes fixed, and we have a steady base to rest upon. We next observe that, in walking, the heel first touches the

Fig. 11.

ground. If the bones of the leg were perpendicular over the part which first touches the ground, we should come down with a sudden jolt, instead of which we descend in a semi-circle, the centre of which is the point of the heel.

And when the toes have come to the ground, we are far from losing the advantages of the structure of the foot, since we stand upon an elastic arch, the hinder extremity of which is the heel, and the anterior the balls of the toes. A finely formed foot should be high in the instep. The walk of opera dancers is neither natural nor beautiful; but the surprising exercises which they perform give to the joints of the foot a freedom of motion almost like that of the hand. We have seen the dancers in their morning exercises stand for twenty minutes on the extremities of their toes, after which the effort is to bend the inner ankle down to the floor, in preparation for the Bolero step. By such unnatural postures and exercises the foot is made unfit for walking, as may be observed in any of the retired dancers and old *figurantes*. By standing so much upon the toes, the human foot is converted to something more resembling that of a quadruped, where the heel never reaches the ground, and where the paw is nothing more than the phalanges of the toes. This arch of the foot, from the

Fig. 12.

heel to the toe, has the astragalus, A, resembling the key-stone of an arch; but, instead of being fixed, as in masonry, it plays freely betwixt two bones, and from these two bones, B and C, a strong elastic ligament is extended, on which the bone A rests, sinking or rising as the weight of the body bears upon it, or is taken off, and this it is enabled to do by the action of the ligament which runs under it.

This is the same elastic ligament which runs extensively along the back of the horse's hind leg and foot, and gives the fine spring to it, but which is sometimes ruptured by the exertion of the animal in a leap, producing irrecoverable lameness.

Having understood that the arch of the foot is perfect from the heel to the toe, we have next to observe that there is an arch from side to side; for when a traverse section is made of the bones of the foot, the exposed surface presents a perfect arch of wedges, regularly formed like the stones of an arch in masonry. If we look down upon the bones of the foot, we shall see that they form a complete circle horizontally, leaving a space in their centre. These bones thus form three different arches—forward, across, and horizontally; they are wedged together, and bound by ligaments, and this is what we alluded to when we said that the foundations of the Eddystone were not laid on a better principle; but our admiration is more excited in observing that the bones of the foot are not only wedged together, like the courses of stone for resistance, but that solidity is combined with elasticity and lightness.

Notwithstanding the mobility of the foot in some positions, yet, when the weight of the body bears directly over it, it becomes immoveable, and the bones of the leg must be fractured before the foot yields.

We shall proceed to explain how the knee joint and hip joint, independently of the exertion of muscles, become firm in the standing position, and when at rest; but before we enter upon this, let us understand the much-talked-of demonstration of Borelli, who explained the manner in which a bird sits upon a branch when asleep. The weight of the creature and the consequent flexion of the limbs drawing the tendons of the talons so as to make them grasp the branch without muscular effort.

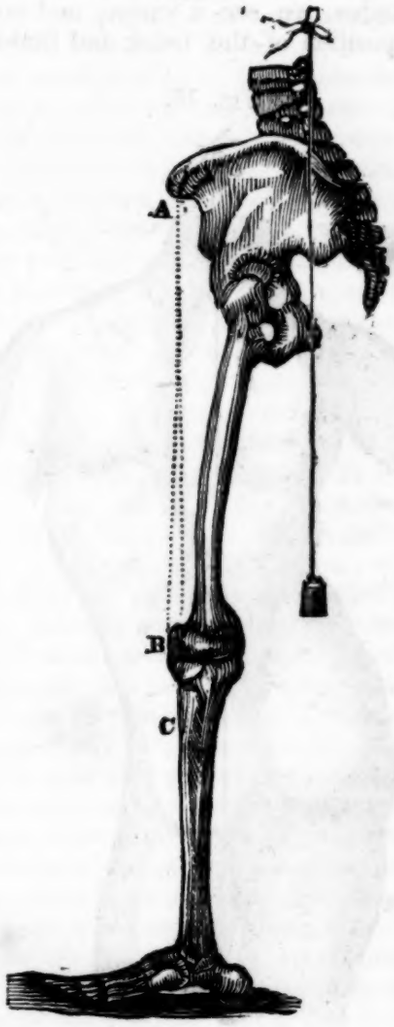
Fig. 13.

The muscle A passing over the joint at B, and then proceeding to the back of the leg, and behind the joint at C, and so descending behind the foot at D, it extends to the talons; and the weight of the bird bending the join' B and C, produces the effect of muscular effort, and makes the claws cling.

But why should the anatomist have recourse to this piece of comparative anatomy, when he has so fine an example in the human body? And one which is much more interesting, as, in fact, it is the foundation of reasoning upon the diseases and accidents of the limb. If this beautiful arrangement in the healthy and perfect structure of a man's limb be not attended to, it would be easy to prove that many important circumstances in regard to disease and accidents must remain obscure.

The posture of a soldier under arms, when his heels are close together and his knees straight, is a condition of painful restraint. Observe, then, the change in the body and limbs, when he is ordered to stand at ease; the firelock falls against his relaxed arms, the right knee is thrown out, and the tension of the ankle joint of the same leg is relieved, whilst he loses an inch and a half of his height, and sinks down upon his left hip. This command to "stand at ease" has a higher authority than the general orders. It is a natural relaxation of all the muscles, which are consequently relieved from a painful state of exertion: and the weight of the body bears so upon the lower extremity, as to support the joints independently of muscular effort. The advantage of this will be understood when we consider that all muscular effort is made at the expense of a living power, which, if excessive, will exhaust and weary a man, whilst the position of rest which we are describing is without effort, and therefore gives perfect relief. And it is this which makes boys and girls, who are out of health and languid, lounge too much in the position of relief, from whence comes permanent distortion. This figure represents the bones of the leg.

Fig. 14.

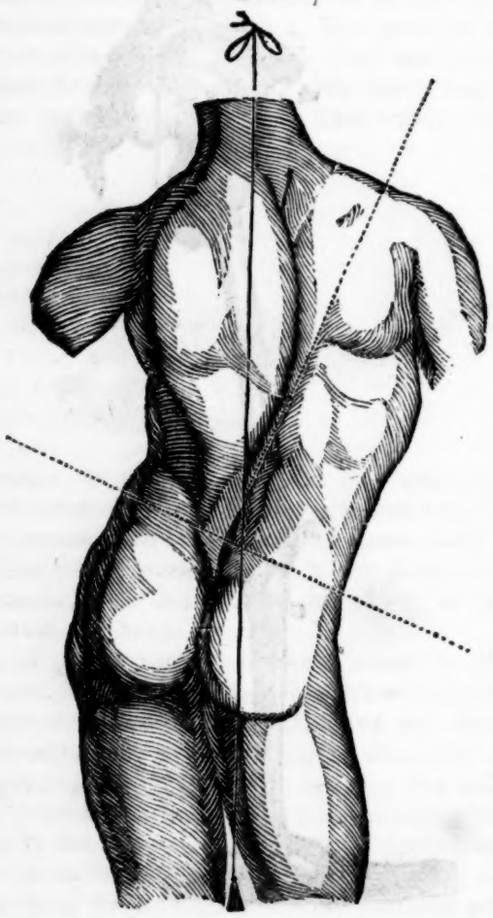


The plumb line shows the direction of the gravitation of the body falling behind the head of the thigh bone. Now, if it be understood that the motions of the trunk are performed on the centre of the head of the

thigh bone, it must follow that the weight of the body in the direction of the plumb line must raise the corner of the haunch bone, at A. From this corner of the bone, a broad and strong band runs down to the knee-pan, B, in the direction of the dotted line. The powerful muscles which extend the leg are attached to the knee-pan, and, through the ligament at C, operate on the bones of the leg, stretching them and preventing the flexion of the joint; but, in the absence of the activity of these muscles, the band reaching from A to B, drawn, as we have said, by the weight of the body, is equivalent to the exertion of the muscles, braces the knee joint, and extends the leg; and we have before seen that the extension of the leg fixes the ankle joint. Thus the limb is made a firm pillar under the weight of the body, without muscular effort.

When the human figure is left to its natural attitudes, we see a variety and contrast in the position of the trunk and limbs.

Fig. 15.



This position of the body resting on the lower extremities throws the trunk into an elegant line, and places the limbs in beautiful contrast, as we see in all the best specimens of sculpture.

Now, that we have understood that the lower extremity becomes in some positions a firm pillar, it is the more necessary to observe the particular form of the head of the thigh bone.

Fig. 16.

It is here seen that the head of the bone A stands off from the shaft by the whole length of the neck of the bone B; the effect of this is, that, as the powerful muscles are attached to the knobs of bone, C, D, they turn the thigh bone round in walking with much greater power than if the head of the bone were on a line with the shaft. They, in fact, acquire a lever power by the distance of D from A, as, during the action of these muscles, the limb is stiff; the rolling of the thigh directs the toe outwards in walking.

When the weight of the body is perpendicular over the ball of the great toe, the whole body is twisted round on that point as on a pivot. This rolling of the body on the ball of the toe, and consequent turning out of the toes in stepping forward, is necessary to the freedom and elasticity of the motion. The form of all the bones of the leg, and the direction of all the muscles of the thigh and leg, combine to this effect. So far is it from being true, as painters affect to say, that the turning out of the toes is the result of the lessons of the dancing master.

A certain squareness in the position of the feet is consistent with strength, as we see in the statues of the Hercules, &c.; but the lightness of a Mercury is indicated by the direction of the toes outwards. In women there would be a defect from the breadth of the pelvis, and a rolling and an awkward

gait would be the consequence; but in them the foot is more turned out,, and a light elastic step balances the defect arising from the form of the pelvis. Any one may be convinced of this by observing people who walk awkwardly, especially if they walk unequally. Look at their feet, and you will see that one foot goes straight forward, whilst the other is turned outwards, and that when they come upon the straight foot, they come down awkwardly, and have no spring from it.

On the Architecture of the City of New-York. By ?. To the Editor of the Mechanics' Magazine.

Sir,—I have been in hopes that the very clever Alphabet of Architecture, which has appeared from time to time in your useful Magazine, would have induced some *original* communications upon this one of the most interesting and ornamental of the fine arts, and one which we must acknowledge with regret has been too little cultivated in our good city. To which of our public buildings can we point with any feeling of pleasure and pride? We have a few, it is true, in which the designs are good as far as they go; but there are in all some things to offend: one is too diminutive; in another the material is of an inferior kind; a third is deformed by the addition of some *fandangle* or other, entirely foreign to the style, after it is supposed to be completed; in short, a correct eye cannot rest with satisfaction upon any one of the *fancy fabrics* with which New-York abounds. To instance a few, let us take a stroll up Wall street. The Exchange first arrests our eye, and is remarkable, I believe, for nothing but its columns, each shaft being a single block, and weighing some 25 tons and upwards. I suppose it answers every purpose of an Exchange, but we can say nothing further in its favor. We are next attracted by the Phenix and National Banks, both well designed, but entirely too small. They seem as if intended rather as samples of the Sing-Sing marble than to answer any useful purpose. Next we have the United States Branch Bank, City Bank, &c.: all flat unmeaning piles of stone and mortar. Turning into Broadway, the City Hall is not without claims to our attention; and considering the date of its construction, and the perfect youthfulness of the art with us at that period, I am far from condemning it *in toto*, as some have done, because, perhaps, the windows may be too numerous, and the cupola too small; the attic story, however, I must confess to be in very bad taste; but the rotunda is a redeeming feature in the building, and has been very deservedly admired. The Hall of Record, on the left, is patched up, perhaps as well as could have been expected; but who could transform a Dutch jail into a Grecian temple? No less a person, certainly, than the chairman of the Committee on Arts and Sciences of the Board of Aldermen of the city of New-York. As we proceed up Broadway we pass a large flat dirty-looking structure, known as the Masonic Hall, and upon which money enough was expended to produce something ornamental to the city; but such a presumptuous idea seems not to have entered into the calculations of the architect. As we advance we must not forget the French Church, a step or two out of our way, in Franklin street, which bids fair to be a well proportioned classical edifice, and, with the exception of the choice of material, reflecting credit upon the architect and the Society. Returning to Broadway, after another mile of little interest, we find ourselves opposite to St. Thomas' Church: to speak correctly, the only specimen of Gothic architecture we possess, the Cathedral being in an unfinished state, (which is to be regretted, as the interior is in a very good style.) This building, with all its arches and tracery, and painted glass, and gloom, is, after all, (I refer to the interior more particularly,) but a pretty piece of fancy work. The architect has allowed his imagination too much range, and committed many faults, which are, however, easily pardoned, when we consider that it is the pioneer of its style in our city. The effect from the street is good, and taken as a whole should be respected. We are now in the upper part of the city, and it is but a step to Lafayette Place; and here we cannot but regret the taste which could think of accommodating a Corinthian colonnade to a row of dwelling houses. No fault can be found with the colonade itself (I do not speak of the basement); the columns are, I dare say, exactly nine and a half modules, according to Palladio, and the members in their proper proportion; but I blame the want of fitness in the arrangement. It is not calculated to answer the proposed end, the principal rooms being badly lighted, and the building not sufficiently disconnected.

There are other works, both public and private, not without merit, but I have confined myself to the most attractive; and if my observations have any virtue in them, I need hardly repeat my first remark, that architecture has been too much neglected in our city. But let us hope that this reproach will not always be cast upon her. In the

erection of the new Custom House an opportunity is offered to redeem herself in the eyes of her sister cities; for, although a Government building, and independent of our municipal authorities, still it must owe its elegance or deformity to the taste, or want of it, of our own citizens and architects, Messrs. Town and Davis; and that it *will* be an ornament to our city, no one can doubt after having seen the elevation and section as exhibited in the American Academy. It is enough in this place to say of it, that it is in a pure classical style, and without any claim to originality—a deficiency, I think, very much in its favor. One thing I rejoice at: the architects have received, I am well informed, positive assurance from the Secretary that they shall be permitted to "*follow their own plan*, without being subject to the revision of any committee or sub-committee." It is well known to what annoyance artists are subject from the impertinent interference of some people in such situations of petty authority, who seem to imagine them mere tools, and with no right to a will of their own. In time, however, these difficulties will disappear, and with the general diffusion of correct taste, men of real talent, whether artists, mechanics, or tradesmen, will find their proper level in society. ?

The Philosopher of Bologna unmasked; or Galvani not the Discoverer of Galvanism. By R. W. Dickinson. [From the London Mechanics' Magazine.]

Sir,—It appears to me very singular that no notice should ever have been taken of an experiment made by M. Du Verney, before the Fellows of the Royal Academy at Paris, in the year 1700, and published by their secretary, the celebrated M. Fontenelle, in his Account of the Transactions of the Society for that year. It is there related that M. Du Verney exhibited a dead frog, and on irritating it *with a scalpel* the nerves of the belly, that led to the thighs and legs, trembled and suffered a sort of convulsion. He afterwards cut the nerves in the belly, and stretching them with his hands, a similar convulsion was produced *by the application of the scalpel*. Now, though it may at this distance of time be impossible to adduce positive proof that Galvani was acquainted with this previous experiment of M. Du Verney, I cannot help thinking that it is *quite as likely* he was so, as that he should have come to a knowledge of the fact in the strange way he pretended, namely, through one of his pupils accidentally touching *with a scalpel* the crural nerve of a frog, which was being prepared in the laboratory of the professor, to make a soup for his sick wife. That a *frog* should have been the animal operated upon in both instances, and a *scalpel* the operating instrument, are coincidences pregnant with suspicion. At all events, this much cannot be disputed, that the Bolognese philosopher did at least only discover what Verney had discovered and made known to the world long before; though there is now, probably, as little chance of our seeing the name of *Verneyism* substituted for *Galvanism*, as *Columbia* for *America*.

I am, sir, yours sincerely,

R. W. Dickinson.

Ilfracombe, Dec. 16, 1833.

A Compendium of Civil Architecture, arranged in Questions and Answers, with Notes, embracing History, the Classics, and the Early Arts, &c. By Robert Brindley, Architect, Surveyor, and Engineer. [Continued from page 85.]

SARACENIC. A. D. 410.

Q. What is said of the Saracenic style of architecture, considered as the immediate precursor to the Gothic?

A. This style, partaking of very little of the aboriginal inhabitants, is distinguished by the lofty boldness of its vaulting, the slenderness of its columns, the peculiar mixed form of its curves, the variety of its capitals, and the immense profusion of its ornaments.

Q. Where is the greatest peculiarity of this style?

A. In its small clustered pillars and pointed arches, formed by the segments of two intersecting circles.

Q. What is the difference between the Egyptian Saracenic and the Spanish Saracenic?

A. Chiefly in the form of the arch; held in comparison by the gate of Cairo, with that of the Alhambru in Granada, and the great church at Cordova.

GOTHIC. A. D. 408.

Q. What is the style of the Gothic architecture?

A. It is grand, characteristic, and impressive, having a peculiarity of claim.

Q. What are the elements of this style?

A. Spires, pinnacles, and lofty pointed windows, derived from its type, the pyramid or cone. Thus Mr. Elmes observes, "all we see in it is pyramidical; its shafts shoot upwards; its arches are shaped like points of lancets; its windows form themselves into pyramidical tracery; and it has not been inaptly compared to groves of trees."

Q. Where are the principal Gothic edifices erected?

A. In the North and West of Europe.

Q. Commencing with Britain, as now partaking of the rudiments of architecture, A. D. 400, how may the variations be delineated?

A. The first epoch of time comprises that space of time between Augustan Excellence and the total expulsion of the Romans, 450.

Q. The second epoch?

A. This commences with the introduction of the Saxon style.

Q. The third epoch?

A. This commences with that of the English, including all its varieties, from the plainness of the Norman to the meretricious enrichments of the florid.

Q. The fourth epoch?

A. That of the Grecian and Roman orders.

Q. What was the condition of the aboriginal Britons?

A. That of barbarism; and their architecture was no less so; for, "The Britons," says Diodorus Siculus, who was contemporary with Cæsar, "dwell in wretched cottages, constructed of wood, and covered with straw. These wooden houses were not square, but circular, with high tapering roofs, having an aperture at the top for the admission of light, and the emission of smoke."

Q. How were their Druidical temples constructed?

A. They consisted of ranges of unhewn stone, enclosing a circular area; their only covering being the canopy of heaven.

Q. Did the Romans introduce their own architecture?

A. Yes, in some degree; of which a variety of specimens have been found, and some few still remain, such as the ramparts on the great Roman road (North of England); several altars and sepulchres, as discovered on Chatham hill, Kent, and at York. The gate of Lincoln is also a fine specimen, the only one retaining its original use.

Q. After the Romans left England, what was the state of architecture.

A. It declined rapidly; their numerous architects being called to the building of Constantinople; and the people left behind having neither skill nor courage to defend themselves from the Anglo-Saxons, every thing fell by their barbarous hands, destroying towers and castles instead of preserving them. Indeed, they knew nothing of the fine arts or architecture for nearly two hundred years after their arrival.

SAXON. 600 A. D.

Q. How is the progress of Saxon architecture described, as noticed principally in ecclesiastical edifices?

A. First, the *early Saxon* or *dawning Saxon*, continued from the conversion of Egbert, King of Kent, about A. D. 598, and from the first building of Archbishop Theodore's churches to the time of King Alfred, about A. D. 872.

Q. What is the criterion distinguishing this era?

A. The *multitude* of the minute and *designedly varied* ornaments of the several parts. A characteristic specimen may be seen in Barefreston church, in Kent.

Q. What is the second description?

A. The *full Saxon*, or *perfect Saxon*, continued from Alfred to Canute, and the time of the first Harold, about 1036.

Q. What is the criterion?

A. The style appears to be more bold and noble, with less numerous ornaments, but still with much variety in the adornments. The cathedral of Christ Church, Oxford, and Canute's gate, at St. Edmund's Bury, Suffolk, afford illustrations.

Q. What is the third era?

A. The *declining Saxon*, liable to be confounded with the Norman; which species of architecture continued from Canute to the conquest.

Q. What are the distinguishing marks?

A. Its clumsy stately magnificence, on a greater scale, and in greater proportions; and by its having cast off so much of its varied ornaments, being difficult to be distinguished from the first style of the Norman.

Q. Where are specimens of the style?

A. At Southwell and Waltham.

DANES. 819 A. D.

Q. What of Danish architecture?

A. It can only be conjectural, from the limited power and pagan disposition of the people.

Q. What specimens does tradition hand down as the work of their hands?

A. The round towers attached to several English parochial churches in Norfolk and Suffolk; but from the rudeness of the works, they are of very remote date. And the conclusion is, that they destroyed rather than built, but in time adopted the architectural fashions of the Anglo-Saxons, as as well as their religion.

Q. What was the result of the Norman conquest?

A. The introduction of a new species of architecture, which may be divided into *four* distinct periods or styles: 1st, Norman style; 2d, early English; 3d, decorated English; 4th, perpendicular English.

Q. To what are these styles to be confined, as affording any criterion for dissertation?

A. Almost wholly to churches. Castella-

ted buildings being so metamorphosed, from the different modes of warfare, as to leave no testing proof of their originality.

Q. What is to be understood by the appellation *style*?

A. The composition of the whole building.

Q. How were the ancient ecclesiastical edifices built?

A. Generally in the form of a cross, with a tower, lantern, or spire, erected at the intersection.

Q. How was the interior divided?

A. Thus: 1st, the space westward of the cross into the *nave*, or as rendered in the Latin, "ampli interioris templi pars;" 2d, the space eastward of the cross—the *choir;* 3d, the parts north and south—the *transepts;* 4th, the divisions outward of the piers—the *aisles.*

Q. Where is the *choir?*

A. In the western part, where the organ is, and generally enclosed by a screen. In cathedrals it does not extend to the eastern end of the building, as there is a space behind the altar usually called the *Lady Chapel;** and furthermore, it is only between the piers, and does not include the *aisles*, which are passages. Sometimes the choir advances west of the cross, as at Westminster.

Q. How describe the *transepts*?

A. They have side aisles, which are often separated by screens for chapels.

Q. What are *chapels?*

A. They are additional buildings distributed throughout.

Q. What and where is the *triforium?*

A. It is a gallery between the arches of the nave and the clerestory windows.

Q. Where is the *chancel?*

A. In churches not collegiate, it is the eastern space about the altar.

Q. What are the *porches?*

A. Small buildings attached to the sides of the main body at the doors, over which are schools, vestries, &c.

Q. What is the *font?*

A. A large stone basin supported by a pedestal; and generally in large churches is placed in the western nave.

Q. Where are the principal entrance doors?

A. In large churches they are generally at the west end, or end of transepts, or both; in small churches at the sides.

Q. What other buildings are attached to most cathedrals?

A. A *Chapter House*—often multangular; and *cloisters* (usually on the same side) forming a quadrangle, with an open space in the centre—the sides of which are a series of open arches. The other wall is the side of the church, or other buildings, with which the cloisters communicate by sundry doors. The cloisters are arched over, and form the principal communication between the different parts of the monastery.

Q. What are the spaces between the windows or apertures termed?

A. *Piers.*

Q. What are the windows above the arches, on the outside, over the top of the aisles, called?

A. *Clerestory windows.*

Q. What are the buildings above the roof designated?

A. *Steeples*; if square topped, *towers;* which may be square, multangular, or round; these are frequently surmounted by *spires,* of conical form, and sometimes a short tower of light work, termed a *lantern.*

Q. What are towers of greater height, in proportion to their diameters, named?

A. *Turrets.* These sometimes have small spires. Large towers often have turrets at their angles.

Q. What are *buttresses?*

A. Projections between the apertures, or at the angles of towers, worked in stages, to strengthen the adjacent parts.

Q. How are the walls crowned?

A. By *parapets*, which are straight at the top, or *battlements*, indented.

Q. What are *arches?*

A. They are either round, pointed, or mixed.

Q. Whence their origin?

A. That of the round is from the Romans; the pointed is of Arabian extraction, introduced by the Crusaders, on returning from the Holy Land; and the mixture is that of English device.

Q. What are *piers?*

A. Columns on shafts, of different sizes and shapes, supporting the several arches.

Q. How many arches are there described?

A. Eight.

Q. What is the first?

A. A *semi-circular arch*, having its centre in the same line with the spring.

Q. What is the second?

A. A *segmental arch*, with the centre lower than the spring.

Q. What is the third?

A. An *equilateral arch*, described from two centres.

Q. What is the fourth?

A. A *drop arch*, which has a radius shorter than the breadth of the arch, and is described about an obtuse angled triangle.

* The Lady Chapel is not always at the east end of the choir. At Durham it is at the west end of the nave; at Ely, on the north side.

Q. What is the fifth?

A. The *lancet arch*, which has a radius longer than the breadth of the arch, and is described about an acute angled triangle.

Q. What is the sixth?

A. The *horse-shoe arch*, which has the centre above the spring.

Q. What is the seventh?

A. The *ogee*, or *concentrated arch*, which has four centres; two in or near the span, and two above it reversed.

Q. What is the eighth?

A. The *tudor arch.* This is flat for its span; has two of its centres in or near the spring, and two considerably below it.

Q. What is the space included between the arch and a square formed outside called?

A. The *spandrill.*

Q. How are windows divided?

A. Perpendicularly by *mullions*, and horizontally by *transoms;* the ornaments at the head into *tracery*, either *flowing* or *perpendicular.*

Q. How are the several parts of the tracery ornamented?

A. With small arches and points termed *feathering*, or *foliation.*

Q. How are small arches ornamented?

A. By *cusps*, enumerated as trefoil, quatrefoil, and cinquefoil, &c. These *cusps* are again feathered, called double feathering.

Q. What are *tablets?*

A. Small projecting mouldings. That at the top, under the battlements, as a *cornice*; that running round doors and windows, the *dripstone*; and if ornamented, a *canopy.*

Q. What are *bands?*

A. Small strings round shafts.

Q. What are *niches?*

A. Small arches sunk in walls, often ornamented, and into which figures are introduced.

Q. What is a *corbel?*

A. A projection from the wall, supporting an arch or a pier.

Q. What is a *pinnacle?*

A. A small spire.

Q. What are *canopies?*

A. Projections surmounting doors, niches, thrones, &c. and beautifully ornamented.

Q. What are the several bunches of foliage on canopies designated?

A. The small, *crockets;* the large, *finials.* These ornaments are also used on pinnacles.

Q. What are the seats for the Dean, Canons, &c. in choirs of collegiate churches, named?

A. *Stalls.*

Q. What is the Bishop's seat called?

A. The *throne*, exceedingly ornamented, with canopy, &c.

Q. As what is the ornamental work over *stalls* known?

A. *Tabernacle work.*

Q. What is the *stoupe?*

A. A basin, containing holy water, placed in a niche near the entrance.

Q. Where is the *rood loft?*

A. In churches, not collegiate, it is a space screened off between the nave and chancel, and which, formerly, contained images.

Q. Where and what is the *piscena?*

A. It is in a niche, near to the altar, distinguished from the stoupe by having a small hole at the bottom, to carry off the remains of the consecrated wine. It is often double, having a place for the bread.

Q. What are *crypts?*

A. Vaulted chapels in the foundations, used for burial places.

Ericsson's Caloric Engine. [From the London Mechanics' Magazine.]

Sir,—A critical notice, or review, of the principle of the Caloric Engine, as described in the short pamphlet from which you made some extracts in a recent number of your Journal, having made its appearance in the *Repertory of Patent Inventions* of this day, you will much oblige me by admitting the following remarks upon it into your columns:

I will follow the reviewer in the same order that he proceeds.

1st. I am reminded of "the Cornwall steam engines raising from 60 to 80,000,000 pounds one foot high by the consumption of a bushel of coals." In reply, I would advise the reviewer to make himself acquainted with the power produced by a *steam engine that consumes only 10 lbs. of fuel per hour.* This, I think, will be his best guide in comparing the effect of the trial engine with that of the steam engine; for I cannot help concluding, from the ignorance with which he has treated the subject, he would waste his time by entering into the philosophy of the two modes of producing motive power, in order to determine which of them is capable of producing the greatest effect by a given quantity of fuel.

2d. The reviewer cannot understand how any loss of heat can take place in the steam engine by the steam being condensed, and its heat imparted to the condensing water; but a few words, I think, will enlighten his mind on this most important point. The water used for condensing is usually about 50°, and the heat of the condenser about 120°; thus, the boiler will be fed with water at 70° higher temperature than if the boiler were *not supplied* from the condenser.

Now the latent heat of steam is 1000°; hence the whole heat returned to the boiler only amounts to $\frac{1}{11}$ part of the heat carried away by the condensing process, and $\frac{12}{11}$ *of the heat used is constantly wasted.* On this ground I contend that the principle of the steam engine involves a misapplication of heat in producing mechanical power. In the caloric engine only a small portion of heat is wasted, or carried away by the cooler, and on that ground I contend that it will produce more power than the steam engine by a given quantity of fuel.

3d. The reviewer states, that "the volume of air cannot be readily lessened when once expanded." This is utterly fallacious, for the actual operation of the regenerator of the trial engine proves just the contrary, and I have it in my power to name a great number of the most eminent scientific men in the kingdom in support of my statement, they having witnessed the actual operation.

4th. The reviewer "suspects" that atmospheric air contained in a close vessel will remain equally *dense* in every part, although of unequal temperature. *I* suspect, that if at one end of that vessel the air be kept at about 480° higher temperature than at the other, it will *there* be at only half *density*, or *specific gravity*, compared to the other end. As to the pressure exerted against the sides of the vessel, no one is so ignorant as not to know that it is equal at every point.

5th. The reviewer is laboring under a strange delusion as to the respective pressures in the *body* of the regenerator, and in its *pipes*. These pressures I have most distinctly stated to be equal to 56 inches column of mercury in the former, and 18 inches in the latter. There being no vacuum about the engine, these columns must, of course, be both subjected to atmospheric pressure, consequently the pressure indicated by them are pressures above the atmospheric pressure. Moreover, to prevent misconception on that part, it has been particularly pointed out, in a note, where *particular pressure on a vacuum* was meant.

6th. The reviewer has been anticipated in his advice to have mercurial gauges communicating with the body of the regenerator, as well as with its pipes, for the pressures stated in the pamphlet are those indicated by gauges now actually attached to the engine.

7th. The reviewer's confused statements with regard to effective pressure and back pressure, and his equally confused conclusions as to the power of the trial engine, need no refutation, since he has proceeded on false data, being ignorant of the rapidity of the transfer of the heat, and of the actual pressures in the various parts of the machine. But I must inform the reviewer that, without "magical effect," the mean resistance against the piston of the cold cylinder is only 14 lbs. to the square inch. If the reviewer ever noticed that when the piston of a blowing cylinder first is moved, it meets with hardly any resistance, but that resistance increases as the piston is pushed farther into the barrel of the pump, he will readily see why the piston of the cold cylinder cannot be opposed by a pressure equal to 19 lbs. to the square inch, until it has performed nearly the half stroke; and he will also, by a short calculation, find that the *mean resistance* throughout the stroke will be very nearly 14 lbs.

8th. With regard to the mean pressure against the working piston, I will only ask the reviewer to consider what would be that resistance, supposing air of 19 lbs. to the square inch was admitted into the working cylinder and the *admission valve closed*, when the piston had performed $\frac{3}{4}$ of the length of the stroke; he will, I am sure, find 17 lbs. will be very near the mark.

9th. The reviewer, instead of allowing himself to be "struck with surprise at the idea of there being a power of 13 horses communicated to the main shaft," should have undertaken to multiply 154, (the square, in inches, of the working piston,) by 17, (mean pressure in pounds,) by 165, (speed per minute in feet.)

10th. The reviewer thinks that the air is "wire-drawn" in the slide passages. This I beg most decidedly to contradict; for all the apertures about the slides are $7\frac{1}{2}$ inches square, which is considerably more, in proportion, than in low pressure steam engines.

I am, sir, yours obediently,

JOHN ERICSSON.

Edward street, Regent's Park.

Farther Illustration of the Principle of Mr. Ericsson's Caloric Engine. [From the London Mechanics' Magazine.]

SIR,—The following remarks, in elucidation of the principle of my Caloric Engine, will, I feel confident, not be unacceptable to your many scientific readers.

To arrive at a clear understanding of the advantage gained by the new mode of employing heat adopted in this engine, it may not be amiss to pause for a moment to consider how heat is at present made use of when employed to actuate that universal instrument of mechanical power, the steam engine. Is it necessary *to the effect produced*, that the heat should be absorbed or destroyed, or in any way diminished in energy?

If this question can be answered in the negative, then it will be quite logical to assume that the power of the steam engine forms but a fraction of that which the combustion of a given quantity of fuel is capable of producing.

Well, then, let us suppose a quantity of steam, of known volume and pressure, to be admitted into a vessel containing cold water of a given weight and temperature; the elevation of temperature which will be produced will, of course, afford an accurate measure of the quantity of heat contained in the steam previous to its condensation. Suppose, now, that an equal volume of steam, of equal pressure, as in the first instance, is admitted under a piston, working in a cylinder, and subjected to a proportionate load; that piston will, of course, move until all the steam has been admitted, and by its motion exert a force proportionate to the pressure of the steam and the volume displaced. Let, then, the steam be discharged from under the piston into the vessel of cold water, under similar circumstances as in the first supposition, and it will be found that the *same* elevation of temperature will take place as when the steam was not previously employed to raise the piston. We thus find that *the production of mechanical force by heat is unaccompanied by any loss of heat.**

But, in the steam engine, this remarkable circumstance is not productive of any advantage, for although nearly all the heat generated in the boiler is unquestionably conducted to the condenser, that heat cannot from thence be brought back to the boiler again for the purpose of raising steam, having in the condensing process been diffused amongst a large quantity of matter, and brought to a much lower temperature than the steam.†

On these grounds the inference seems incontestible, that the steam engine is not constructed on a correct physical principle, inasmuch as it consumes a greater quantity of that precious commodity, fuel, than is necessary for the production of the mechanical force obtained.

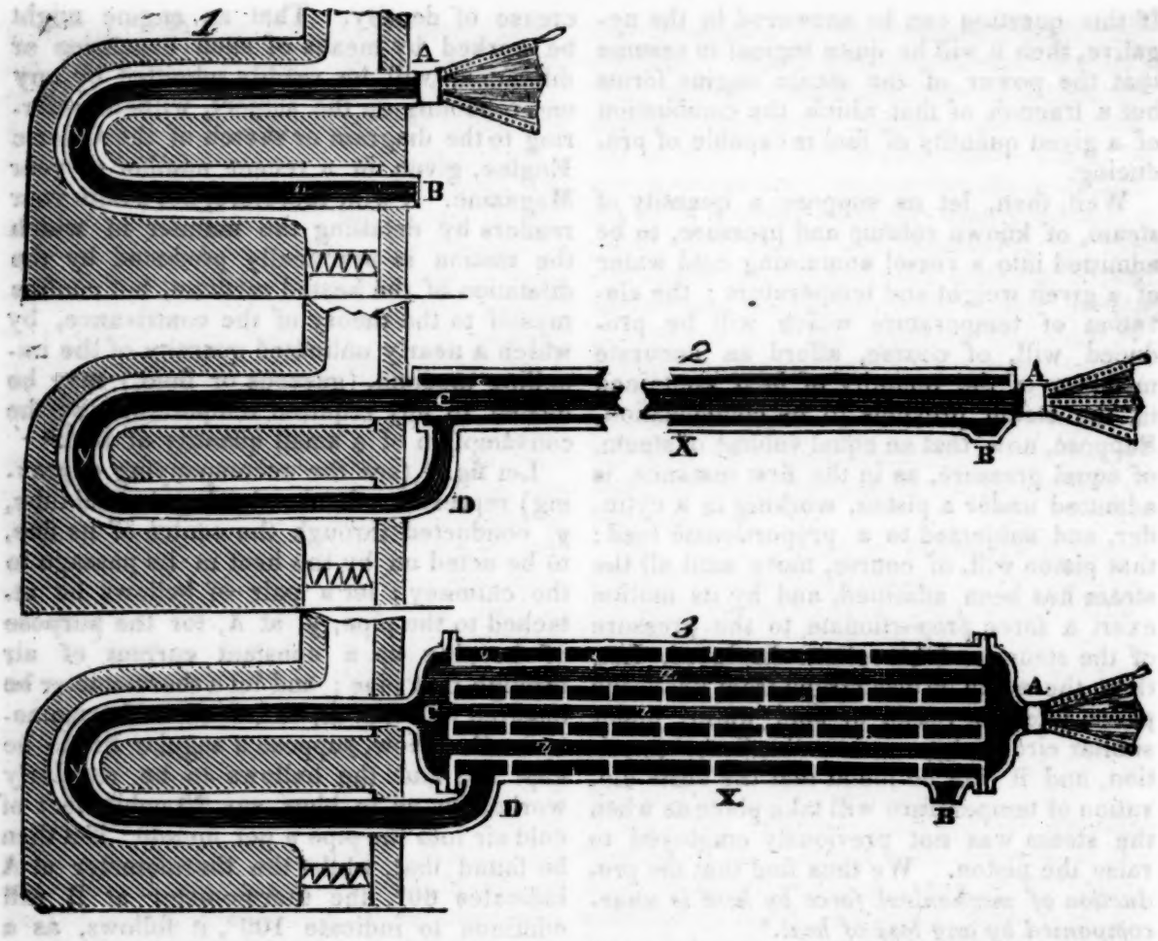
It is well known that all fluid substances, the gases particularly, expand very considerably by being exposed to the action of heat, and that, if kept in a state of compression previous to being heated, their expansive force will, at a given temperature, be greater, and that in the same proportion as the increase of density. That an engine might be worked by means of such expansion or dilatation, will be readily admitted by any one reflecting on the subject, without referring to the diagram or sketch of the Caloric Engine, given in a recent number of your Magazine. I will, therefore, not detain your readers by detailing the manner in which the motion is practically produced by the dilatation of the heated medium, but confine myself to the theory of the contrivance, by which a nearly unlimited quantity of the impelling medium, (gaseous or fluid,) may be heated to any required temperature, by the consumption of a small quantity of fuel.*

Let fig. 1 (see the accompanying engraving) represent a furnace having a metal tube, *y*, conducted through the centre of its flue, to be acted on by the heat in its passage to the chimney; let a pair of bellows be attached to the pipe, *y*, at A, for the purpose of keeping up a constant current of air through that pipe; and let a thermometer be inserted into it at A, and another thermometer at B. Now, suppose a regular fire to be kept up, and the bellows to be regularly worked so as to blow, say 20 cubic feet of cold air into the pipe *y* per minute: if it then be found that, whilst the thermometer at A indicates 60°, the thermometer at B will continue to indicate 100°, it follows, as a matter of course, that the heat transmitted by the furnace per minute will be accurately ascertained by calculating what quantity of heat is required to raise 20 cubic feet of air from 60 up to 100°. Now suppose the same furnace, with its metal tube *y*, to be represented by fig. 2, but instead of having the bellows attached to the metal tube, suppose them to be attached to a pipe, A C, of infinite length, and let this pipe be inclosed in a casing, X; suppose, further, this casing to be surrounded by a perfect non-conductor of heat, and instead of allowing the hot air to pass off directly, as at B in fig. 1, let it be conducted from the metal tube *y*, through the pipe D, into the casing X, and pass off at B. Then let thermometers be inserted in the pipes at A, C, D, and B, the bellows being worked at the same speed as before, and an equal fire kept up. At the commencement, the thermometer at A and at C will, of course, both indicate 60°, but the thermome-

* Losses by radiation need not here be taken into account, for they do not affect the theory.

† Of course, every boiler is fed from the condenser, but this produces a saving of fuel of only one-thirteenth part of the whole quantity consumed: hence thirteen-fourteenths of the heat generated is constantly wasted.

* The journal cited in my last communication having, by some strange oversight, mistaken the Caloric Engine for an "Air Engine," it will be well to direct the attention of your readers to the fact, that various gaseous and even fluid substances, capable of considerable dilatation by heat, are equally applicable for using the heat over and over again, and for the reason that the *impelling agent may be varied*, while, in every case, *caloric is indispensable*, has the term *Caloric* Engine been chosen.



ter at C will very soon begin to rise, on account of the heat conveyed into the casing X; but any increase of temperature at C will, of course, cause an increase of temperature at D. This again will still further increase the temperature at C, and so on in continued succession, until the thermometer at D indicates a temperature nearly equal to that of the hot air in the beginning of the flue leading from the furnace: any further increase of temperature, of course, cannot take place. Now, since the quantity, or rather weight, of air forced through the metal tube *y* is the same as in the first proposition, and the power of the fire likewise, this latter proposition, illustrated by fig. 2, incontrovertibly proves that the *temperature* to which the air may be brought is made perfectly independent of the *quantity* of heat generated in the furnace.

But the *quantity* of air to be heated will also be equally independent of the *quantity* of heat generated: for suppose that, in the first proposition, the draught be checked so as to diminish the consumption of fuel $\frac{3}{4}$, then the 20 cubic feet of air constantly circulated per minute will be raised about 10°, instead of 40°; but apply the contrivance for bringing the heat back, as illustrated in fig. 2, and the thermometers at C and D will be affected just as above described, except that more time will be required before the temperature at D is brought to the full height, and that less heat will ultimately escape at B. Thus it may be proved *theoretically*, that any quantity of fluid air or gaseous matter can be heated up to a high temperature, independently of the quantity of heat actually generated for that purpose. Although this is apparently a paradox, it is not so; for by referring to the illustration in figs. 2 and 3, it will at once be seen that the circulating fluid is of a *high temperature only when passing the point* D, and that it gradually diminishes in temperature as it recedes, and gradually increases as it advances towards that point. However, for the purpose of obtaining mechanical force this is quite as advantageous as if the fluid retained its high temperature when it escapes; for at the point D is the heated fluid admitted into the working cylinder, and from thence passed off into the casing X. The manner in which this is done, your diagram of the Caloric Engine, in a former number, fully explains.

Fig. 3 represents the form of an apparatus used in practice; its operation is precisely the same as in fig. 2, and thermometers placed at A, C, D, and B, will indicate temperatures proving the increase of temperature and transfer of heat in a similar manner.

The cold fluid is forced into the furnace through a number of small tubes, Z, and the hot air is passed off through the vessel X, called the regenerator. The currents, both in this vessel and in the tubes, are broken in a peculiar manner, so as to produce a constant intermixture of particles, which is absolutely necessary for effecting a rapid transfer of heat. But to such an extent has this object been attained by the contrivances instituted, that hot air, constantly passed at the rate of 6 feet per second, through a pipe 1¼ inch bore, fourteen feet long, and entering at a temperature of 300°, has, by a counter-current of *equal magnitude*, been brought down to 85°, the counter-current at the same time entering at 72°.

I remain, sir, yours, &c.

J. Ericsson.

Edward st., Regent's Park, Jan. 7, 1834.

Meteoric Phenomenon. By S. Boyden. To the Editor of the Mechanics' Magazine.

Sir,—I read with pleasure the remarks of Mr. Strong, in your Magazine, on the phenomenon of the 13th of November. If accurate observations have been taken from places at any considerable distance apart, the distance of the meteors from the earth may be computed. I saw them from four o'clock, as long as they were visible. I agree with you, that the point from which they appeared to diverge advanced to the west, but remained the same with regard to the stars; this point was near the 20th degree of Leo, 5 degrees north of the Ecliptic, which was 20 degrees south of the zenith, at 30 minutes past 6 o'clock, being near the point in the heavens towards which the earth was going in its orbit. If the shoal of meteors through which the earth has passed were nearly at rest, the motion of the earth would give to the meteors the appearance they presented of diverging from the point the earth was advancing to; and if they were not equally electrified with the earth's atmosphere, they must become luminous on entering it, and remain so until the equilibrium was restored, leaving the excited electricity in the atmosphere through which they had passed, to return to rest and cease to be luminous. I am not informed that any of them touched the earth, or to what extent they have been seen, though probably they were visible in the parallel of west longitude, between 30 and 160 degrees, or to more than one third of the earth.

Hoping to receive further information, I remain yours respectfully,

Seth Boyden, Engineer.

Newark, N. J., Feb. 15, 1834.

On the Location of Railroad Curvatures, being an investigation of the principal formulas required for field operations in laying curves to pass through given points. By J. S. Van De Graaff. To the Editor of the American Railroad Journal.

Sir,—Having been personally engaged in tracing the various curves for railroad lines, and being frequently under the necessity of making calculations relative to divergent curves and tangents, under a variety of existing circumstances, I was induced to enter into an investigation of certain formulas, embracing the principal cases which usually occur in practice. These formulas, the chief of which I believe to be new, are now offered to you for insertion in your Journal. This is done only with a view to the possibility of adding a small mite to the general stock of information upon that subject; for nothing can be better calculated to produce such an effect than a comparison of the different methods of computation which have been resorted to by different individuals engaged in the solution of such problems. A full investigation of this subject, embracing all the variety of cases which occur in the field, would require too much room in your columns, and I have therefore consulted brevity as much as perspicuity would allow. Very respectfully,

J. S. Van De Graaff.

Art. 1. Any two points being fixed in the general direction of a route, through which it is proposed to lay a line of railroad composed of several curves and tangent lines, the cost of construction is in most cases not the only requisite datum to fix the definite location of the intermediate points,—for there are very few varieties of ground, except in a very broken country, which will not admit of several different lines connecting the same points, all at nearly an equal expense of construction, and all within the same *limits* of curvature. It then becomes an object to make such a selection of right lines and curves, and such a distribution thereof, as will produce, *at a given expense*, the most efficient road.

But no particular rule can be given as a guide for the judicious arrangement of a line; and the general object here proposed is an investigation of such formulas as are required in the field. Let any two rectangular co-ordinate axes be assumed, and take any number of *equal* straight lines, originating at the origin of the co-ordinate axes, and connected together at their extremities in such a manner as that, when each point of connection is joined by a straight line to the origin, these latter lines will form, with one of the co-ordinate axes, a series of angles in *arithmetical progression*. It then follows, agreeably to the principles of elementary geometry, that each point of connection of those *equal* straight lines, will be situated in the cir-

cumference of a circle, passing through the origin, and touching that co-ordinate axis from which the arithmetical series of angles is counted. And this obviously suggests the common method of tracing a circular arc in the field by means of a chain whose length is each of those *equal* straight lines, and an instrument for measuring the arithmetical series of angles from the tangent line.

In all the formulas which will be deduced in the course of the present inquiry, it must be remembered that all measurements of distance are supposed to be made in chains, or decimal parts of a chain. The chain will therefore be the *unity* of length, and may have any value whatever; but as this will be a constant quantity in the field, it follows that the *curvature* of a line, traced in the manner just described, can only be made variable by different arithmetical series of angles. The *common difference* of these series of angles will therefore be called the *moduli* of curvatures, and will always be denoted by a letter T. That co-ordinate axis which coincides with the tangent line will be designated as the axis of x; the other being that of the ordinate y; and any curve will be considered given, or found, when the modulus of curvature is given, or found.

2. *Let a given curve be traced from the origin of a system of rectangular co-ordinate axes, agreeably to the preceding article; it is proposed to investigate formulas which will express the values of the co-ordinates of the station at the extremity of the* nth *chain.*

The inclinations of the different chains to the axis of x, in succession from the origin, are respectively T, 3T, 5T, &c.; and consequently their projections upon the co-ordinate axes are obviously cos. T, cos. 3T, cos. 5T, &c., and sin. T, sin. 3T, sin. 5T, &c. respectively. Hence, by taking the sums of those projections, the following equations will result:

$$x = \text{cos. T} + \text{cos. 3T} + \text{cos. 5T} + \text{\&c. . . . . cos.} \{\text{T} \cdot \overline{2n-1}\}$$

$$y = \text{sin. T.} + \text{sin. 3T} + \text{sin. 5T} + \text{\&c. . . . . sin.} \{\text{T} \cdot \overline{2n-1}\}$$

The last term in each of these two series will obviously be the nth term; and the sum of n terms of each series being taken, agreeably to the known principles of analytical trigonometry, the following formulas will be the result:

$$x = \frac{\text{sin. } 2n\text{T}}{2 \text{ sin. T}}$$

$$y = \frac{1 - \text{cos. } 2n\text{T}}{2 \text{ sin. T}}.$$

These are the expressions required; and they have that form which is most convenient for computation from a table of natural sines and cosines.

3. *To find the radius of curvature.*

It is sufficiently evident, that when n is made *variable*, the maximum value of x will be the radius of the described circle; but when x is a maximum, it follows that sin. $2n$T will also be a maximum when T remains constant, as will readily appear from the expressions obtained in the last article. Now, the maximum value of sin. $2n$T is unity, and consequently denoting the radius of the described circle by R, the result is,

$$\text{R} = \frac{1}{2 \text{ sin. T}}.$$

4. *Take two rectangular co-ordinate axes, having their origin at a given station in a tangent line, from which a certain required curve is to be laid, passing through a point designated by the co-ordinates* x, y; *the given tangent line coinciding with the axis of* x. *Parallel respectively to each of the co-ordinate axes, let any number of rectangular lines be traced from the origin, and terminating in the point designated for the required curve to meet; these rectangular lines being selected in any convenient manner to pass any obstacle which may happen to occur. Let the algebraic sums of each of these rectangular lines be taken, agreeably to the axis to which they are respectively parallel. These sums will obviously give the values of the co-ordinates* x, y; *and from thence it is proposed to determine a formula expressing the value of the modulus of curvature of the required curve.*

Let each of the two formulas obtained in art. 2 be squared; and let the second of the two formulas be then divided by the sum of the squares. The following expression will be the immediate result:

$$\text{Sin. T} = \frac{y}{x^2 + y^2}.$$

And thus the modulus of curvature of the required curve becomes known. This is one of the most important formulas used in the field; for it can be applied under any circumstances, when the designated point is not visible from the origin. If that point can be seen from the origin, the curve sought is usually obtained in a more simple manner.

5. *Suppose two curves to be laid upon the same tangent line, and take* b *to denote the distance between their origins. Let one of those curves have a given modulus of curvature denoted by* T, *and let it pass through a given point at the extremity of the* nth *chain. It is then proposed to find the modulus of curvature of the other curve such that it may also pass through the same given point.*

The co-ordinates of the given point, taken with reference to the origin of the required curve, will obviously be $x \pm b$, and y. Hence taking T′ to denote the required modulus, it follows from the last article that, sin. T′ $= \frac{y}{\overline{x \pm b}^2 + y^2}$. Now, substituting in this expression, for x, y, their values obtained in art. 2, and the necessary reductions being made, the following formula will be the result:

$$\text{Sin T}' = \frac{1 - \text{cos. } 2n\text{T}}{\frac{1 - \text{cos. } 2n\text{T}}{\text{sin. T}} \pm 2b \text{ sin. } 2n\text{T} + 2b^2 \text{. sin. T}}$$

This theorem will be found to be quite convenient in the field to answer several different purposes, when the curves are too long to be within the limits of the more simple approximative methods. With regard to the double sign, it must be observed that the *negative* value obtains, when the origin of the curve sought is in *advance* of the origin of the given curve; and the positive value must be taken in the contrary case.

6. If the point designated for the required curve to meet does not coincide with the extremity of the *n*th chain of the given curve, as in art. 5 it is supposed, but varies a small distance to the right or left; yet, *if the curves are long*, the best method will always be to compute the value of T′, as though the required curve were intended to pass through the point considered in that article; and then the requisite *small variation* in the computed value of T′, to meet the case proposed, may be subsequently calculated by very simple approximative methods.

7. *Suppose two given curves to be laid upon the same tangent line, and let* b *denote the distance between their origins. Take* T *and* T′ *to represent the given moduli of curvatures; and* n *and* m *the number of chains contained in each curve respectively. It is proposed to determine the distance between the extreme stations of those two curves, and the inclination of the two tangents at those two extremities.*

I will here take T to denote the modulus of curvature of that curve which is most in advance upon the tangent line, with respect to the directions in which the curves are laid from their origins. Let x, y, and x', y', be the respective co-ordinates of the two extreme stations, each originating at the origin of its respective curve. The difference of co-ordinates, when referred to any common origin on the tangent line, will then be $x + b - x'$, and $y - y'$. Hence, taking w to denote the distance sought, the common principle of analytical geometry gives the following expression:

$$w = \{\overline{x+b-x'}|^2 + \overline{y-y'}|^2\}^{\frac{1}{2}}$$

And from this equation the required distance becomes known; for, by art. 2,

$$\left.\begin{aligned} x &= \frac{\text{sin. } 2n\text{T}}{2 \text{ sin. T}} \\ y &= \frac{1-\text{cos. } 2n\text{T}}{2 \text{ sin. T}} \end{aligned}\right\} \qquad \left.\begin{aligned} x' &= \frac{\text{sin. } 2m\text{T}'}{2 \text{ sin. T}'} \\ y' &= \frac{1-\text{cos. } 2m\text{T}'}{2 \text{ sin. T}'} \end{aligned}\right\}$$

The inclination of each tangent to the common tangent at the origins is $2n$T and $2m$T′ respectively, and $2n$T—$2m$T′ will consequently express the required inclination of the two tangents to each other. These two tangents will converge when $2m$T′ is less than $2n$T, and the determination of their point of intersection will be easily effected by the preceding principles.

8. When the two moduli of curvatures T and T′ are equal, the problem considered in the last article becomes much more simple. For the value of w being developed, and the values of x, y, and x', y', substituted for them, and the necessary reductions being made agreeably to the known principles of analytical trigonometry, the following formula is the result:

$$w = \left\{ \frac{1-\text{cos.}\{2n\text{T}-2m\text{T}\}}{1-\text{cos. } 2\text{T}} + b \times \frac{\text{sin. } 2n\text{T} - \text{sin. } 2m\text{T}}{\text{sin. T}} + b^2 \right\}^{\frac{1}{2}}$$

This expression will be useful for a variety of purposes in the field.

9. The formulas which have been investigated in the 8 preceding articles are all *rigorously accurate;* and when long curves are under consideration, which embrace portions of circumferences containing more than 20° or 30°, these formulas will find their application; for, in such cases, none of the *usual methods* of approximation are applicable.

The above very brief view of this subject will be only sufficient to illustrate the general fact, that all the necessary formulas are easily made to flow from the two principal formulas given in art. 2. Very useful approximative rules may also be easily derived from those already given; but I cannot continue this inquiry. The subject may perhaps be resumed in some future number of this Journal.

VAN DE GRAAFF.

Lexington, Ky., February, 1834.

ENGINEER'S MANUAL.—We publish the following extract from the preface of a forthcoming work, believing that in so doing we shall confer a favor on many of our readers. The author of the work is a practical engineer, engaged upon one of our western railroads, and has often furnished us with valuable communications for the Magazine, signed "V. D. G." We hope to be able to make further extracts from the work itself, by which those most interested in it may be able to judge of its utility to the profession.

In presenting the present little work to the notice of the public, the author is actuated with the hope of contributing in a small degree to the collection of such principles as are daily required in locating railroad curvatures with ease, accuracy, and despatch. The author has not possessed an opportunity of ascertaining extensively what may be the various methods of calculation at present resorted to by scientific and skillful engineers generally, in determining the relative positions of given points in the different curves and tangent lines which frequently come under consideration in the field; and the principal formulas which are here offered to the public, embracing these cases, are therefore *such only* as have been used with advantage by himself. The only methods of calculation which he knows to be *now in use by others*, and which are sufficiently commodious for purposes in the field, are of an approximative kind; and under circumstances which often occur, the use of those approxi-

mative results is attended with inconvenience and delay. It is therefore hoped that the different formulæ which will be here found investigated and arranged for use will be acceptable to those who may not be in possession of more convenient methods of computation, which are sufficiently rigorous to pass a *long curve* through a given point at the first attempt.

The second part of this Manual is devoted to the methods of constructing the elliptical curve. It is well known that the true curve of an ellipsis is produced by an oblique section of a cylindrical surface, whether that surface be circular or elliptical; and hence it is that the true figure of an ellipsis should be traced with precision in the construction of skew bridges, having either circular or elliptical arches. Other cases in the department of a civil engineer might be mentioned, in which a perfect ellipsis is required; and there is no method available in practice hitherto published, which is known to the author, for constructing an oval, consisting of given circular arcs, and approaching with sufficient precision to the curve of an ellipsis.

A table of natural sines and cosines is to the engineer almost of daily use. Such a table is therefore subjoined to this work. It is also thought desirable to add a table of the square and square roots of numbers; which not only very frequently saves labor, but will also be the means of diminishing the liability to err in numerical calculations. Indeed, those two tables, or others of a similar kind, will be considered indispensable to the skillful prosecution of all field operations, by those *who know* the facilities which may be derived from them.

Estimated Expense of Construction of the Wood Work of Railroads. By E. Johnson. To the Editor, &c.

Sir,—Agreeably to your request, I forward the estimated expense of construction of the wood work on the plan suggested in my communication. (See page 121.)

Reported expense to the directors of the Buffalo and Black Rock Railroad Company:

10560 feet, lineal measure, of round timber hewed on one side, at 2½ cts.	$264 00
36960 feet of plank, $12 per M.	443 52
7040 feet oak scantling, 2 by 4 inch, $7,	49 28
2640 lbs. spike, 8 in. (4 to the lb.) 8½ cts.	224 40
16 tons iron, 2 in. by ½ in., $55,	880 00
Additional security at the end by sills and iron, distribution of materials, incidentals, &c.	100 00
Labor in putting down road, (sills placed in the grade by the contractor for grading,)	84 00
Expense per mile,	$2045 20

The estimated cost of the construction of the Tonewanda railroad from Rochester to Attica, 43 miles, on this plan, with sills 12 inches in diameter, plank 2½ inches thick, 20 tons of iron per mile, spike 3 to the pound, as reported to the directors, is, per mile, $2544 63.

The principal saving in the expense is in the grading. Where the surface conforms nearly to the level of the road, the line is cleared, the sills placed in their proper position, and ample ditches cut to form the grade. More than half of the distance of the Tonewanda railroad line is of this description, and can be prepared at a small expense per mile, in consequence of the change in the form of the wood work.

I have made arrangements for putting down with lime, in April, one mile on the Buffalo road, to be done in the following form, viz.: using 2½ bushels of stone lime per rod, 1½ bushels slacked and placed on the sills and over the surface of the grade before putting down the plank; one bushel made into grout and applied to the surface of the planking when the road is completed, covering the surface and filling the joints of the timber, and then covered with sand:

Requiring 800 bush. per mile, at 10 cts,	$80 00
Labor, - - - - - -	20 00
Expense per mile,	$100 00

You have anticipated on this form of road the use of common carriages. This manner of using the road was submitted to the commissioners of the Tonewanda railroad in November last, with a view principally of using the first twenty miles from Rochester for market teams, allowing them to enter the road at certain hours in the forenoon, and afternoon to return, and so arranged as to not interfere with trains of cars; which would require the track to be five feet wide, in place of four feet nine inches, the usual width, which would not be materially objectionable. This question must be decided by experiment and circumstances.

I am, respectfully, yours,

Elisha Johnson, Civil Engineer.

Rochester, Feb. 28, 1834.

Mr. Nutt's System of Bee Management. [From a Friend of this distinguished Apiarian.]

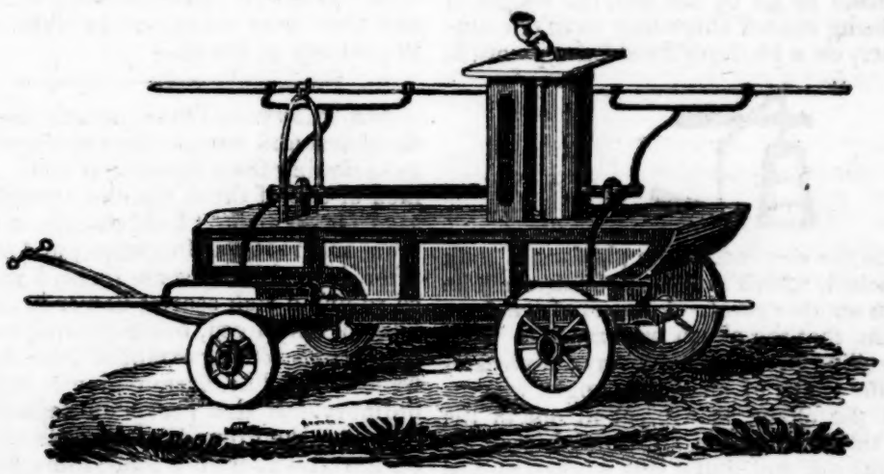
It is one of the least interesting pleasures of an editor's duty to record the successful achievement of designs to which he has devoted his helping hand, to place in successful practice and deserved estimation the efforts of inventive genius, to whatever part of the wide fields of research its attention may have been devoted; and multifarious as are the objects which address themselves to his attention, in his varied capacity, as treasurer of scientific improvement and discovery, or dispenser of their benefits, it is ever a source of gratification that those exertions have been requited with merited success.

Since the first promulgation of Mr. Nutt's views, the Mechanics' Magazine has been the vehicle for the publication of his plans, and the furtherance of his designs. That they were possessed of originality and merit was evident, by their requiring neither sophistry

nor mystery to impress them on public attention, for they claimed regard more from their simplicity than the extension at which they aimed. The pages of this Journal were alike open to the results of his patient investigation and research, as well as the queries of his opponents, and we are confident that, even to those proficient in ordinary modes of Bee Management, or who adhered with pertinacity to their usual modes, much good was elicited.

To the Mechanics' Magazine Mr. Nutt returns his obligations for the impetus which, through its medium, has been given to his system, the remuneration which he has received for his labors, and the great and extended success which has in every instance attended its introduction.

The system has now become one of facts, which are now sufficiently numerous, not to prove its superiority over all others which have preceded it, but to render its introduction a matter of important, national, economical consideration. To recapitulate individual instances of its success were only to refer to what has been extensively published in this journal. The superiority of the honey and its chemical characters are based upon the same foundation, and we therefore feel that in taking leave of the subject as a theory, or its success as merely problematical, by placing it upon permanent record, it will only be necessary to give such instances of its established success, as will at once silence the objections of rivalry. An inspection of the exhibition of the varied collections this season, at the National Gallery of Practical Science, in Adelaide street, where it has stood the test of the strictest examination, and has been the subject of much attention and inquiry, will sufficiently prove the correctness of this observation. The results of Mr. Nutt's takings, during the present year, from six colonies, has been seven hundred pounds of honey, averaging from one hundred to one hundred and twenty pounds from each hive; nor has he been alone in these marked proofs of success, as a reference may be made to the Apiaries of the Marquis of Blandford, Delabere House, near Reading; Rev. Thomas Clark, Gedney Hill; John Burman, Esq., Wisbech; and J. D. Salmon, Esq., Stoke Ferry, Norfolk, where the average products have been the same.



ROGERS' PATENT BALANCE FIRE ENGINE. —Engines of this description are manufactured at Waterford, Saratoga county, New-York, on an extensive scale, and the company do not hesitate to recommend it to the public as decidedly superior to any fire engine now known. It has been tested by skilful engineers at the United States Arsenals at Gibbonsville and Washington, and by them pronounced to be superior to any other. The Government Arsenal at Washington is now supplied with one of said engines, and the proprietors have on hand a contract with the government for the supply of all their principal arsenals.

This engine combines the advantage of more power at less cost than any other in use, and needs only to be examined and tried to insure it a preference to all others.

We have much pleasure in inserting the following certificate:

"I have this day superintended a trial of a fire engine of Rogers' patent, made by William Platt & Co. at Waterford, Saratoga county, New-York, with one of the best Engines belonging to the Corporation of the city of New-York. The trial was made in the Corporation Yard, and from the experiment, I am fully persuaded that the engine of Rogers' patent will throw more water, and at a greater distance, with the application of less power than our Corporation en-

gines. No fire engine has ever been exhibited in this city which could compete with the New-York engines, or which would answer our purpose, except Rogers' patent above mentioned. The construction of Rogers' patent is exceedingly simple, and I see no reason why it should not go into general use. Besides, it has another advantage—it can be furnished at much less cost.

"JAMES GULICK,
"Chief Engineer N. Y. Fire Dep."

DUTCH CHURNS.—When the churning process was going on, I have observed the outside of the churn wrapped over with cloth, which I was told was for the purpose of keeping the outside clean. The Dutch have various ways of working the churn. At the large dairies it is generally with a horse; this I observed near Delft, at Ter Leide, near Leyden, and near Leuwarden, in Vriesland; at others they are churned by the hand, by turning a large fly-wheel,

1

(figure 1.) At Almenaar, near Harlingen, I saw a churn made to go by the feet, the weight of the body being moved alternately from one side to the other, on a platform fixed on a pivot (fi-

2

gure 2); to the one end of the platform a stalk being attached, which moved the churn-handle attached to another pivot. I observed at a farm near Gouda, that the churn was made to work from the ceiling in a very easy way; a piece of wood, in the shape of an obtuse angle, was attached at the elbow to a pivot in one of the beams of the ceiling; the churn-stick was attached to the one end, and it was worked up and down by the hand at the other end, (figure 3.)

3

And in North Holland they churn by means of dogs in wheels, in a similar way to the turnspits.—[Quarterly Journal of Agriculture.]

LUMINOUS PLANTS.—In the case of the rhizomorphæ there can be no mistake. These curious plants are found in subterranean cellars and mines, and illumine the darkness which surrounds them with their magic light. In some of the coal mines of Dresden they are singularly beautiful and brilliant. Mr. James Ryan once informed me that he was accidentally shut up in a mine, and the light of one of the rhizomorphæ was so brilliant that he could see distinctly to read a letter by it. As the rhizomorphæ prey on dead wood, they impart to it a phosphorescent light. The rhizomorphæ phosphorentia is found in the mines of Hesse, and yields light in the dark, but ceases to be phosphorescent in hydrogen and some other gases: the rhizomorphæ subterranea and acidula have also been found to illumine the mine with their fairy light. Mr. Erdmann thus describes the luminous appearance of the rhizomorphæ in one of the mines of Dresden: "I saw luminous plants here in wonderful beauty; the impression produced by the spectacle I shall never forget. It appeared, on descending into the mine, as if we were entering an enchanted castle. The abundance of these plants was so great, that the roof, the walls, and the pillars, were entirely covered with them, and the beautiful light they cast about almost dazzled the eye. The light they give out is like a faint moonshine, so that two persons, near to each other, could readily distinguish one another. The light appears to be most considerable when the temperature of the mines is comparatively high." That the light is electric seems most probable, when we consider that an electric discharge imparts phosphorescence to Canton's phosphorus, (calcined shells,) and that heat enhances the light.—[Murray's Physiology of Plants.]

AIR PLANTS.—These attach themselves to the driest and most sapless surface, and flower as if issuing from the richest soils. "A specimen of one of these, which I thought curious," says Dr. Walsh, "I threw into my portmanteau, where it was forgotten; and some months after, in unfolding some linen, I was astonished to find a rich scarlet flower in full blow; it had not only lived, but vegetated and blossomed, though so long excluded from light, air and humidity. The barren pine is not less extraordinary. It also grows on sapless trees, and never on the ground. Its seeds are furnished, on the crown, with a long, filmy fibre, like the thread of a gossamer. As they ripen they are detached, and driven with the wind, having the long thread streaming behind them. When they meet with the obstruction of a withered branch, the thread is caught, and, revolving round the seed, at length comes into fixed contact with the surface, where it soon vegetates, and supplies the naked arm with a new foliage. In Brazil it grows like the common plant of a pine apple, and shoots from its centre a long spike of bright scarlet blossoms. In some species, the leaves are protuberant below, and form vessels like pitchers, which catch and retain the rain water, furnishing cool and refreshing draughts to the heated traveller, in heights where no water is to be found. The quantity of this fluid is sometimes very considerable, and those who have attempted to reach the

flower-stem have been often drenched by upsetting the plant.

Capacity of Bodies for Water.—As it may be interesting to many to know the comparative as well as the positive absorption of water by various bodies, we subjoin the following table, the details of which were made with care. The weight of each substance was ascertained before immersion; next, when the water ceased running and began to drop; and, lastly, when all dropping had ceased, and the bodies were in that state in which they may be supposed to be full of moisture.

	Dry.	*Dripping.*	*Done dripping.*
Flannel.....	144 grs.	1553 grs.	700 grs.
Woollen Cloth..	56 "	370 "	191 "
Linen	375 "	2110 "	1050 "
Calico.....	115 "	1150 "	450 "
Cambric Muslin.	95 "	883 "	307 "
Very fine do.	54 "	715 "	297 "
Glove Leather..	106 "	1170 "	677 "
Kid do. ..	172 "	770 "	421 "
Shoe do. ..	95 "	194 "	177 "
Sponge.....	185 "	2440 "	2070 "

From these data the following table may be constructed, to show in the first instance the absorbing powers, and, in the second place, the retaining powers, for moisture, of the various bodies thus experimented upon:

Flannel absorbed	11	and retained	5	times its weight of water
Woollen Cloth	6½	"	3½	"
Linen Cloth	5½	"	3	"
Calico	10	"	4	"
Cambric Muslin	9	"	3½	"
Fine Muslin	13	"	5	"
Glove Leather	11	"	6½	"
Kid do.	4½	"	2½	"
Shoe do.	2	"	2	less a fraction
Sponge	13	"	11	"

From these results, it may be seen that, although some substances, in the first instance, take up an equal or nearly an equal quantity of water with the sponge, such as the flannel, fine muslin, and glove leather, yet their powers of retaining the same are very far inferior.

Dip and Declination of the Needle in America.—A manufacturer of compasses at Birmingham would feel much obliged to any of our American or other correspondents, who would supply answers to the following queries: What is the dip and declination of the needle in New-York? and the extent of the variation throughout that and the other states of the North American Union?—[Mec. Mag.]

Magnetic Pole.—We understand that the position of the Magnetic Pole is now finally ascertained by our adventurous countryman, Captain Ross. He has actually been on the spot where the dipping needle becomes vertical, or points straight downwards; while the horizontal needle, having, as it were, no longer any thing to point towards, remains indifferently in any direction given it.—[Athenæum.]

Captain Ross has ascertained beyond question that the Magnetic Pole is nearly in 70° N. lat., and 97° W. long., being 2° of lat., and 3° of long. different from what it was said to be by Captain Parry's observations.—[Naval and Military Gazette.]

Percussion Locks for the Army.—A committee, consisting of three officers of artillery, is now actively engaged at Woolwich in a course of practical experiments, of which the object is to ascertain the propriety, or otherwise, of introducing percussion locks for the army, in lieu of the present flint and steel. No report is, we hear, to be made to government till the firing of 24,000 rounds of cartridges shall have afforded grounds for a decided opinion.

Preservation of Substances by means of Alkalis.—M. Payen has preserved, during many months, polished instruments of iron and steel, by keeping them in solutions of potash and soda,—saturated solutions diluted with one, two, or three times their weight of water. He at first thought that the preserving power depended upon the disappearance of the air, and the carbonic acid in the alkaline mixture, but he afterwards concluded that alkalinity acted an essential part in the phenomenon. In fact, a very small quantity of alkali is sufficient; thus, $\frac{1}{2000}$ and even $\frac{1}{3000}$ of caustic potash in water will preserve from oxidation bars of iron, &c. immersed in it. Lime water, diluted with its own weight of water, or of course without dilution, answers the same purpose. Alkaline carbonates and borax have the same effect, but they must necessarily be stronger.—[Revue Encyclopedique.]

Domestic Silk Handkerchiefs, the product of the native mulberry, have been manufactured in Dayton, Ohio, which are said to exceed the imported ones in durability.

Method of dressing Skins practised in Marocco.—The following account of the method practised in dressing skins in Marocco was transmitted to the Zoological Society by W. Willshire, Esq., a Corresponding Member of that Society, in a letter dated Mogadore, May 5, 1833. Its results are stated to be excellent, as regards the preservation and color of the fur, and the flexibility of the pelt.

Wash the skin in fresh water to deprive it of the salt; as soon as this is done, scrape the flesh off, when take two pounds of alum, one quart of buttermilk, and two or three handfuls of barley-meal, which mix well together, and lay on the fleshy side of the skin equally; fold up and press it together carefully, and let it lie two days. On the third day take it to the sea-side, wash the skin well, and when clean and free from mixture, hang it up to let the water run from it: then take two pounds of alum finely powdered, and throw or spread it equally on all parts of the skin; again fold up as before, and allow it to lie three days, when it will be in a proper state to dry in the sun, laid flat, without taking away the powder. When it is dry, take a pint or two of fresh water, and sprinkle it upon the skin, and again fold it up carefully for about two hours, to imbibe the water; then lay it on a table, and, after scraping it free from the mixture and flesh, take a sand-stone (rather rough) and rub the skin well until it becomes

soft and pliable, then hang it in the shade to dry. The process is then complete.

When the skin is perfect, having the head, horns, &c. take off the horns, and fill their cavity with a mixture of equal parts of powdered alum and ashes of charcoal dissolved in water, and expose them two days to the sun. Saturate the trunks of the horns with eight ounces of alum dissolved in water, and fold up with the skin, and apply the same on each occasion when employed in curing the skin. The flesh on the head and jaws to be carefully taken off, filling the same with powdered alum. It should remain in the sun until perfectly dry.

In addition to the foregoing description of the mode used in Marocco, in dressing skins, as related by the persons employed by Mr. Willshire, it may be well to observe that the process does not take so long at Mogadore, as Mr. W. has often received back skins of the Aoudad and Leopard from the dresser, on the third or fourth, and never exceeding the fifth day, perfectly cured. Allowance has been made by the dresser, in the foregoing description, for the difference in the climate of London.

The skins of smaller animals must not be subjected to so lengthened a process, or they will become harsh, and the pelt impoverished. —[Proceedings Zoolog. Soc.]

Apparatus for obtaining Fire.—A very ingenious apparatus is now exhibiting at the store of Mr. John Bailey, in Union street, for obtaining fire. It consists of two glass cylinders, the outer one of which contains a compound of sulphuric acid and water, and in the inner one, which is without a bottom, is suspended a piece of zinc. The action of the acid upon the zinc creates a gas, which is let out by means of a valve, and, in coming in contact with atmospheric air, immediately ignites a piece of platina exposed to it. The apparatus is very neat, and was constructed by a young man in Mr. Bailey's employ, from a description of a similar work in Europe.—[New-Bedford Gazette.]

Recipes.—*For an Olive Green.* Let the article be first washed in soap and water, then wetted out in warm water; then boil two ounces of chipped logwood and three ounces of chipped fustic together for half an hour; dip out your dye liquor, and put it into a pan with hot water; put in your goods; dissolve two drachms of verdigris in a teacup-full of warm water, which put into a pan of cold water; take your gown from the dye, and run it through the verdigris water, well handling it for ten minutes; take it out and wash it in clean water, and through the dye liquor, and again in the verdigris water, and so continue this process till you obtain the color required, only taking care to wash it out of the verdigris water before you put it in the dye liquor: dry it in the shade.

For Yellow Cotton.—To make a lemon yellow, first wash your article well in soap and water, then rinse it in warm water. For every yard of stout cotton, dissolve a piece of blue vitriol as large as a horse bean, in boiling water; and when the water is at a hand-heat, put the cotton in, and handle it for half an hour. In the interim take a quarter of a pound of weld for every yard of cotton, and boil it well for half an hour; dip the liquor out in a pan, and handle your cotton through this till it comes to the fullness required; take it out to cool, and when cold, wash it out, and dry it in the air.

To preserve Books.—A few drops of any perfumed oil will secure libraries from the consuming effects of mould and damp. Russian leather, which is perfumed with the tar of the birch tree, never moulders; and merchants suffer large bales of this leather to remain in the London docks, knowing that it cannot sustain any injury from damp. This manner of preserving books with perfumed oil was known to the ancients. The Romans used oil of cedar to preserve valuable MSS. Hence the expression used by Horace, "*Digna cedro*," meaning any work worthy of being anointed with cedar oil, or, in other words, worthy of being preserved and remembered.—[Greenf. Gaz.]

To preserve Eggs.—Apply with a brush a solution of gum-arabic to the shells, or immerse the eggs therein; let them dry, and afterwards pack them in dry charcoal dust. This prevents their being affected by any alterations of temperature.

To make Maple Sugar.—It has been customary to cut a gash in the tree, from which saccharine liquor flows, or to bore a hole, and put in a reed, and, when the liquor ceases to flow, plugging up the hole. Both these methods are injurious, and tend to destroy the tree. In the latter case, the tree roots round the plug to some distance within. The following method is proposed in lieu of these, and has been successfully practised in Kentucky. At the proper season for running of the liquor, open the ground and select a tender root, about the size of one or two fingers; cut off the end, and raise the root sufficiently out of the ground to turn the cut end into the receiver. It will emit the liquor from the wound as freely as by either of the other methods. When it ceases to flow bury the roots again, and the tree will not be hurt.

To restore Tainted Beef.—In the last fall I procured an acquaintance of mine in the country to put up a barrel of fat beef for my family's use during the winter. The barrel of beef was sent to me agreeably to contract; but before I had used one quarter part of it I observed it tainted, and so much so as to smell quite offensive. The beef being very fat and fine I was loth to throw it away. I made the following experiment: I procured a half bushel of charcoal, and after taking out the beef and throwing away the offensive pickle, I re-packed it in the barrel, laying the pieces of charcoal between the pieces; and making a new pickle, and adding a little saltpetre, I covered the beef, and in about six days found it as sweet and good as it was when first put up.